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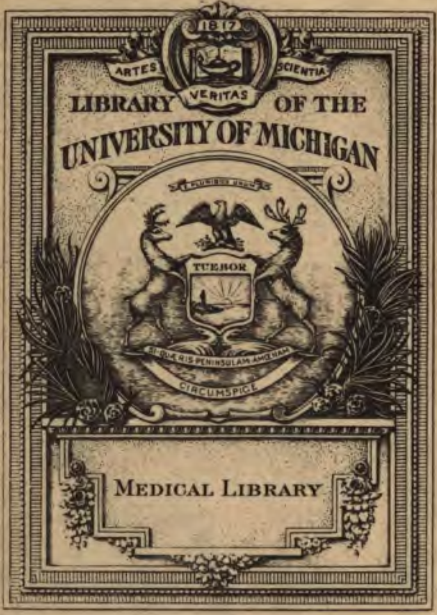

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EDINBURGH
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EXHIBITING

A CONCISE VIEW

OF THE

LATEST AND MOST IMPORTANT DISCOVERIES

IN

MEDICINE, SURGERY, AND
PHARMACY.

VOLUME THIRTY-THIRD.

1830.

UNIV. & BELL. HOSP. MEDICAL COLLEGE,
READING ROOM.

EDINBURGH:
PRINTED FOR ADAM BLACK, NORTH BRIDGE:
LONGMAN, REES, ORME, BROWN AND GREEN, LONDON: AND
JOHN CUMMING, AND HODGES AND MACARTHUR, DUBLIN.

1830.

PRINTED BY JOHN STARK, EDINBURGH.

THE

EDINBURGH
MEDICAL AND SURGICAL JOURNAL,

No. CII.

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The following Works have been received:—

The Principles of Military Surgery, comprising Observations on the Arrangement, Police, and Practice of Hospitals, and on the History, Treatment, and Anomalies of Variola and Syphilis. Illustrated with cases and dissections. By John Hennen, M. D. F. R. S. E. Inspector of Military Hospitals. 3d Edition, with Life of the Author. By his son, Dr John Hennen. London, 1829. 8vo. Pp. 583.

Notions on the Nature of Fever and of Nervous Action. By William Forrester Bow, M. D. Senior Physician to the Alnwick Dispensary, &c. &c. London, 1829. 8vo. Pp. 100.

Elements of General Anatomy, containing an Outline of the Organization of the Human Body. By R. D. Grainger, Lecturer on Anatomy and Physiology. London, 1829. 8vo. Pp. 526.

A Manual of General Anatomy, or a Concise Description of the Primitive Tissues and Systems which compose the Organs in Man. By A. L. J. Bayle, D. M. P. &c. &c. and H. Holland, D. M. P. &c. &c. Translated from the French by Henry Storer. London, 1829. 12mo. Pp. 318.

A Manual of Midwifery, or a Summary of the Science and Practice of Obstetric Medicine. By Michael Ryan, M. D. Member of the Association, &c. &c. 2d Edition. London, 1829. 12mo. Pp. 353.

A Treatise on Neuralgic Diseases, dependent upon Irritation of the Spinal Marrow and Ganglia of the Sympathetic Nerve. By Thomas Pridgin Teale, M. R. C. S. London, &c. Lond. 1829. 8vo. Pp. 120.

Elements of Physics, or Natural Philosophy, General and Medical, explained independently of Technical Mathematics. In Two Volumes. Vol. II. Part I. comprehending the subjects of Heat and Light. By Neil Arnott, M. D. of Royal College of Physicians. Lond. 1829. 8vo. Pp. 320.

Transactions of the Medico-Botanical Society of London. Vol. I. Part I. 8vo. Pp. 78. London, 1829.

The Properties and Preparation of the Rio Negro Sarsaparilla, and of the Angustura Bark, practically examined. By John Hancock, M. D. Fellow of the Medico-Botanical Society, &c. &c. London, 1829.

An Introduction to Medical Botany, illustrated with coloured Figures. By Thomas Castle, F. L. S. M. R. C. S. &c. Lond. 1829. 12mo. Pp. 172.

Hospital Facts and Observations, illustrative of the Efficacy of the New Remedies, Strychnia, Brucia, Acetate of Morphia, Veratria, Iodine, &c. in several Morbid Conditions of the System; with a Comparative view of the Treatment of Chorea, and some Cases of Diabetes. A Report of the Efficacy of Sulphureous Fumigations in Diseases of the Skin, Chronic Rheumatism, &c. By John Lomax Bardsley, M. D. Physician to the Manchester Infirmary, &c. &c. &c. London, 1830. 8vo. Pp. 223.

Plates of the Arteries of the Human Body; after Frederic Tiedemann, Professor of Anatomy and Physiology in the University of Heidelberg. Engraved by E. Mitchell, under the superintendency of Thomas Wharton Jones, surgeon. The Explanatory References translated from the Original Latin, with additional notes, by Dr Knox, Lecturer on Anatomy. Edinburgh, 1829. No. 1, 2, and 3. 4to. Containing the usual and unusual Origin of the Arteries issuing from the Arch of the Aorta, and the Distribution of the Coronary Arteries.

The Anatomy of the Bones of the Human Body; represented in a series of Engravings, copied from the elegant Tables of Sue and Albinus. By Edward Mitchell, Engraver. With Explanatory References by the late John Barclay, M. D. F. R. S. E. Lecturer on Anatomy, Fellow of the Royal College of Physicians, &c. &c. A New Edition by R. Knox, M.D. F. R. S. E. Lecturer on Anatomy, F. R. C. Surgeons, &c. &c. Successor to Dr Barclay. Edinburgh, 1829. 4to. No. 1 to 13 inclusive, containing in 21 plates the Bones of the Head, Chest, upper Extremities, &c.

A Code of Medical Regulations for the Honourable East India Company's Establishment of Surgeons belonging to the Presidency of Prince of Wales' Island, Singapore, and Malacca. Drawn up at the express desire of Government by W. E. F. Conwell, B. L. M. B. and M. D. of Paris, &c. &c. Singapore Mission Press, 1828. 8vo. Pp. 85.

Observations chiefly on Pulmonary Disease in India, and an Essay on the Use of the Stethoscope. By W. E. F. Conwell, B. L., B. S., D. M. P., Surgeon of the Madras Establishment, &c. Malacca, 1828. 4to. Pp. 200.

A New Method of Treating Burns and Scalds, and certain Cutaneous Eruptions. By Michael Ward, M. D. F. R. C. S. L. &c. &c. Manchester, 1829. 8vo. Pp. 83.

The American Journal of the Medical Sciences, No. VIII. August, 1829. Philadelphia, 1829.

The North American Medical and Surgical Journal. Published by the Kappa Lambda Association of the United States, No. XIV. April 1829; No. XV. July 1829; No. XVI. October 1829.

The Medico-Chirurgical Review and Journal of Practical Medicine. Edited by James Johnson, M. D. Physician to the Duke of Clarence, No. 22. New Series, October 1829.

Recherches Anatomiques, Pathologiques, et Therapeutiques sur la Maladie connue sons les noms de Gastro-Enterite, Fievre putride, adynamique, ataxique, typhoide, &c. &c. comparée avec les Maladies Aigues les plus ordinaires. Par P. Ch. A. Louis, Docteur en Medecine des Facultes de Paris, 2 vols. 1829. Baillière, Paris and London.

Flora Gallica, seu enumeratio Plantarum in Gallia sponte nascentium, secundum Linnæanum systema digestarum, addita familiarum naturalium synopsis. Auctore T. L. A. Loiseleur Deslongchamps. Editio secunda, aucta et emendata, cum tabulis xxxi. Baillière, Paris and London, 1828.

Anatomie Pathologique du Corps Humain, ou descriptions avec figures lithographiées des diverses alterations morbides dont le corps Humaine est susceptible. Par J. Cruveilhier, Professeur d'Anatomie à la Faculte de Medecine de la Maison Royale de Sante. &c. containing six plates and five sheets of letter-press. To be published every six weeks. Folio. Baillière, Paris and London.

Principes de Physiologie Médicale. Par Isid. Bourdon, de l'Academie Royale de Medecine des Dispensaires. 2 vols. 8vo. Baillière, Paris and London.

Traité Elémentaire de l'Art des Accouchemens, ou Principes de Tokologie et d'Embryologie. Par Alf. A. L. M. Velpeau, D. M. P. Agrégé a la Faculté de Medecine. 2 vols. 8vo. Baillière, Paris and Lond. 1829.

Traité Elémentaire de Matière Médicale. Par F. S. Ratier, Docteur en Medecine de la Faculté de Paris. 2 vols. 8vo. Baillière, Paris and London, 1829.

Des Causes Morales et Physiques des Maladies Mentales, et de quelques autres affections nerveuses, telles que l'Hystérie, la Nymphomanie, et de Satyriasis. Par F. Voisin, Docteur en Medécine de la Faculté de Paris. 8vo. Bailliere, Paris and London, 1829.

Pharmacopée Universelle, ou conspectus des Pharmacopées d'Amsterdam, Anvers, Dublin, Edinbourg, Ferrare, Geneve, Londres, Oldenbourg, Wurzbourg, Américaine, Autricienne, Batave, Belge, Danoise, Espagnole, Finlandaise, Française, Hanovrienne, Polonaise, Portugaise, Prussienne, Russe, Sarde, Saxonne, Swédoise, et Wurtemburgeoise, des Dispensaires de Brunswick, de Fulde, de la Hesse, de la Lippe, et du Palatinat, des Pharmacopées Militaires de Danemarck, de France, de Prusse, et de Wurzbourg, de la Pharmacopée des pauvres de Hambourg, des formulaires et Pharmacopées d'Augustin, Bories, Brera, Brugnatelli, Cadet de Gassicourt, Cox, Ellis, Hufeland, Magendie, Piderit, Pierguin, Ratier, Saunders, Sainte-Marie, Spielmann, Swediaur, et Van Mons. Par A. J. L. Jourdain. Paris, 1828. Baillière, London. 2 vols. 8vo.

THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. JANUARY 1830.

PART I.

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TABLE II.—Showing the Variations at different places in the absolute Mortality from different Diseases.

No. 1.—Diseases of Brain and Nerves.

	Pos. No.	Proportion to General Mortality. Total.	1 to
Lond. bills, 1795 -1800,	27427	113420	4.13
Do. 1809–1812,	17705	52035	3.
Do. 1820–25,	24195	111935	4.6
Paris bills, 1826,	2062	23786	11
Do. 1818,	3532	19455	5.5
New York, 1816-26,	1125	25645	22
Willan, 1795-96-98,	29	393	14
Clifton for 8 yrs.	29	211	7
Blane,	55	756	13
Morgan,	410	: 1930	4.7
Army, Reg. 1809,	6	: 91	15
Woollcombe,	18	: 321	18
Lond. Physicians,	84	: 1149	13
Manchester below 10 years, by *Roberton*,	490	: 2056 or : 4112	9
Recent Plym.	77	: 552	7.2
Ply. Nav. Hosp.	6	: 368	61
Edin. 1821–24,	91	: 572	6.2

No. 2, *a.*—Apoplexy and Palsy.

	Pos. Nos.	Proportion to General Mortality. Total.	1 to
Lond. bills, 1795 -1800,	1945		58
Do. 1809–12,	1301		40
Do. 1820–25,	2574		43
Paris, 1826,	979		24
Willan,	19		21
Clifton,	13		16
New York,	1125		22
Blane,	23		33
Morgan,	359		5.4
Lond. physicians,	42		27
Woollcombe,	10		32
Recent Plym.	19		29
Plym. N. Hosp.	4		92
Edinburgh,	15		38
Prussia,	85	1123	13.2

No. 2, *b.*—Convulsio.

Paris, 1826,	1772, all infants.		13.4
Lond. 1795-1800	24329		4.7
Do. 1809–12,	14353		3.6

No. 2, *b.*—Convulsio.
(Continued.)

		Proportion to General Mortality.	
	Pos. Nos.	Total.	1 to
Lond. 1820–25,	17074		6
Willan,	2		186
Blane,	4	: 382	95
Morgan,	11		175
Woollcombe,	5		64
Recent Plym.	13		42
Edinburgh,	13		44
N. Hosp. Plym.	1		
Manchester,	332		12

No. 2, *c*—Water in the Brain.

London, 1795,	444		255
Do. 1809,	1100		48
Do. 1820,	3029		37
Willan,	11		36
Clifton,	13		16
Manchester,	125		33
Glas. 1806-1812,			48
Woollcombe,	4		80
Recent Plym.	34		16
Edinburgh,	61		9.5

No. 3.—Lungs, Heart, and Air Passages.

London, 1795,	34232		3.3
Do. 1809,	22802		2.3
Do. 1820,	31926		3.5
Paris, 1826,	7582		3.2
Willan,	136		3
Blane,	196		3.8
Plym. N. Hosp.	206		1.8
Lond. physicians,	332		3.6
Woollcombe,	123		2.6
Recent Plym.	234		2.3
Edinburgh Disp.	211		2.7
Army, Portsm.	50	: 496	9.9
Do. Ferguson,	210	: 1165	5.5
Morgan,	445		4.3
Exeter,	36	: 99	2.8
Clifton,	90	: 211	2.3
India,	11	: 423	39
New York,	10774		2.4
Devonport,	37	: 96	2.6
Manchester,	477		9

No. 3, *a.*—Pulmonary Consumption.

London, 1795,	29864		3.9
Do. 1809,	19693		2.9
Do. 1820,	26260		4.2
Paris, 1826,	3135		7.6
Willan,	119		3.3
Blane,	68	: 382	5.6
Lond. physicians,	187	: 775	4
Woollcombe,	77		4
Recent Plym.	63		8.7
Edinburgh,	93		6
Plym. N. Hosp.	117		3
Prussia,	270	: 1123	4
New York,	7466	: 25645	3
Manchester,	18		228
Army Reg. Ports.	17	: 91	5.4

No. 3, *a.*—Pulmonary Consumption.
(Continued.)

		Proportion to General Mortality.	
	Pos. No.	Total.	1 to
Morgan,	153		13
Clifton,	54		4
Truro,	75 in 100		1.3
In Negros,	100	: 718	7
Philadelphia,			7

No. 4.—Heart and Sudden Death.

Paris, 1826,	777		31
London, 1795, including Apoplexy,	6		
London, 1809, do.	29		
Do. 1820,	1018		109
Willan,	1		
Blane,	6	: 382	64
Woollcombe,	1		
Recent Plym.	13		42
Edinburgh,	13		44
Clifton,	3		70
Morgan,	44		44

No. 5.—Mouth and Stomach.

Lond. bills, 1795	399		298
Do. 1809	397		131
Do. 1820	632		176
Paris, 1826,	1834		13
New York,	861		29
Morgan,	29		68
Clifton,	4		52
Willan,	18		22
Blane,	7		108
Lond. physicians,	25		46
Manchester,	18		225
Woollcombe,	1		
Recent Plym.	24		23
Plym. N. Hosp.	31		12
Edinburgh,	11		52

No. 6.—Intestines and Abdominal Cavity.

London bills,	5568		20
Do.	3194		16
Do.	5196		21
Paris,	2777		9
Willan,	52		7.5
Blane,	69		11
Lond. physicians,	121	: 1149	9
Devonport, with the Stomach,	15	: 96	6
Woollcombe,	31		10
Recent Plym.	111		5
Edinburgh,	84		7
Morgan,	139		14
Ferguson,	440	: 1165	2.6
Plym. N. Hosp.	45		8
Manchester,	164		28
India,	69	: 423	6
Clifton,	28		8
Exeter,	19		5
New York,	4765		5
Army Portsmouth, 1809,	262	: 496	2

No. 7.—Liver.

		Proportion to General Mortality.	
	Pos. Nos.	Total.	1 to
Lond. bills, 1795	433		261
Do. 1809	230		226
Do. 1820	917		122
Paris, 1826,			
Willan,	23		17
Blane,	15		50
Lond. physicians,	38		30
Manchester,	5		822
Recent Plym.	9		61
Plym. N. Hosp.	6		61
Edinburgh,	17		34
Clifton,	6		35
New York,	528		49
India,	5	: 423	84
Morgan,	79		24

No. 8 —Cancer.

London, 1795,	365		310
Do. 1809.	282		198
Do. 1820,	516		217
Paris,	660		39
Willan,	11		36
Blane,	9		84
London Phys.	20		57
Morgan,	25		77

No. 9.—Urinary Organs.

London, 1795,	132		860
Do. 1809,	64		813
Do. 1820,	105		1066
Willan,	2		196
Blane,	9		84
Woollcombe,	4		78
Recent Plym.	9		61
Edinburgh,	3		190
Clifton,	1		
Morgan,	59		33
Manchester,	1		

No. 10.—Sexual Female and Childbed.

London, 1795,	1006		102
Do. 1809,	675		77
Do. 1820,	1188		93
Willan,	12		33
Blane,	26		29
Woollcombe,	13		25
Clifton,	5		42

No. 11.—Throat and Air-passages.

London, 1795,	195		598
Do. 1809,	400		130
Do. 1820,	654		171
Paris,	282		85
Willan,	3		132
Blane,	7		108
Edinburgh,	15		38
Woollcombe,	1		
Recent Plym.	10		55
Manchester,	41		100
Clifton,	2		105

No. 11.—Throat and Air-passages.
(Continued.)

		Proportion to General Mortality.	
	Pos. Nos.	Total.	1 to
New York,	1239		20
Exeter,	5		19
Morgan,	3		643

No. 12.—Skin and Febrile.

Lond. bills, 1795	34		3338
Do. 1809,	19		2844
Do. 1820,	81		1381
Willan,	1		
Blane,	3		252
Woollcombe,	1		
Recent Plym.	2		276

No. 13.—Abortion and Hemorrhagies.

Lond. bills, 1795	116		970
Do. 1809,	120		412
Do. 1820,	195		598
Willan,	14		28
Blane,	5	: 382	76
Lond. physicians,	19	: 775	40

No. 14.—Erysipelas.

Blane,	3		252
Recent Plym.	5		110
Edinburgh,	8		71
Plym. N.Hosp.	1		
Morgan,	10		193
London, 1795,	11		10310
Do. 1809,	12		4336
Do. 1820,	104		1076

No. 15.—Scarlet Fever.

London, 1795,	81		1400
Do. 1809,	21		2472
Do. 1820,	68		1646
Willan,	9		44
Blane,	12		63
Woollcombe,	5		64
Edinburgh,	13		44

No. 16.—Measles.

London, 1795,	1671		74
Do. 1809,	1802		28
Do. 1820,	4261		26
New York,	552		46
Paris,	408		58
Willan,	8		59
Blane,	3		252
Woollcombe,	6		54
Clifton,	9		24
Manchester,	299		14
Recent Plym.	15		37
Plym. N. Hosp.	5		73
Edinburgh,	29		20

No. 17.—Small-pox.

London, 1795,	10867		10.4
Do. 1809,	4399		12
Do. 1820,	4702		24

No. 17.—Small-pox. *(Continued.)*

		Proportion to General Mortality.	
	Pos. Nos.	Total.	1 to
Paris,	187		127
New York, 1816–26,			
But 1826–27,	198	: 4500	28
Clifton,	2		105
Willan,	24		17
Blane,	15		50
Lond. physicians,	39		29
Woollcombe,	46		7
Recent Plym.	25		22
Edinburgh,	45		12

No. 18.—Hooping-Cough.

Lond. bills, 1795	2463		46
Do. 1809	2034		25
Do. 1820	4011		28
New York,	721		35
Willan,	6		61
Blane,	5		155
Clifton,	2		105
Woollcombe,	9		36
Recent Plymouth,	12		46
Edinburgh,	22		26
Manchester,	150		27

No. 19.—Fevers, (continued.)

London, 1795,	11274		10
Do. 1809,	3894		13
Do. 1820,	5829		19
Paris,	1177		20
Willan,	33		12
Blane,	94		8
Lond. physicians,	127		9
Woollcombe,	56		5.7
Recent Plymouth,	31		18
Edinburgh,	14		40
Plym. N. Hosp.	89		6
Manchester,	21		196
Morgan,	251		8
Army Portsmouth,	139	: 496	3.6
Ferguson,	515	: 165	2.3
Clifton,	15		14
Exeter,	27		3.8
New York,	3205		8
India,	328	: 423	1.3

No. 20.—Inflammations.

Lond. 1795, &c.	2633		43
Do. 1809,	2426		21
Do. 1820,	10367		10
Paris,	6308		4
Willan,	84		4.6
Recent Plym.	200		2.7

No. 20, *a.*—Mortification.

London, 1795,	1407		80
Do. 1809,	772		69
Do. 1820,	1224		90

No. 20, *b.*—Abscess.

		Proportion to General Mortality.	
	Pos. Nos.	Total.	1 to
London, 1795,	185		618
Do. 1809,	204		254
Do. 1820,	548		204

No. 21.—Dropsy.

London, 1795,	5249		21
Do. 1809,	2981		19
Do. 1820,	4785		23
New York,	3044		8
Morgan,	137		14
Willan,	33		12
Blane,	101		7
Clifton,	13		16
Exeter,	5		19
Woollcombe,	13		26
Recent Plym.	43		13
Plym. N. Hosp.	14		26

No. 22.—Teething, and Acute Infantile.

London, 1795,	2334		46
Do. 1809,	1330		40
Do. 1820,	1462		78
Paris,	1098		22
Willan,	28		14
Clifton,	4	: 110	27
Manchester,	181		22
Edinburgh,	7		81
Glasg. 1806–12,			48

No. 23.—Scrofula.

London, 1795,	29		3911
Do. 1809,	23		2263
Do. 1820,	65		1722
Clifton,	5		42
Willan,	21		19
Blane,	4		189
Woollcombe,	9		36
Recent Plym.	7		79
Edinburgh,	16		36

No. 24.—Rheumatism and Gout.

London, 1795,	668		170
Do. 1809,	161		323
Do. 1820,	270		415
Morgan,	26		79
Clifton,	2		105
Exeter,	2		49
Blane,	17		45
Recent Plym.	2		276
Plym. N. Hosp.	5		73

No. 25.—Aged.

Morgan,	262		7
Willan,	7	: 245	35
Blane,	4	: 382	95
Plym. Poor-house,	18	: 127	7
London, 1795,	8155		14
Do. 1809,	5629		9
Do. 1820,	12159		9

Notes on Table II.

Dr Wells has remarked, in the Medical and Chirurgical Transactions, Vol. iii. that "Fashion has influenced the formation of the bills of mortality, as several old titles have disappeared, and new ones have been added. Those of one century denominate certain diseases declines, which those of another would call differently." I have therefore attempted to verify or correct the proportion in the London bills by that given by different medical observers; and although there are many discrepancies, there are likewise so many accordant ratios as to entitle them to consideration. There is also to be found an argument for the general accuracy of the bills in the accordance in the numbers of those diseases taken together, which are known to be peculiarly prevalent at particular ages, with the total numbers ascertained to have died at those very ages.

2. *a.* At Paris more apoplexy is said to have happened in the ten years after 1804 than before it; and 1670 males were affected to 627 females.

2. *b.* Fallen of late in the bills; yet higher in the middle period. Its inferior proportion at Paris and Manchester than in London would indicate that *adults* are comprehended in the English bills. Singularly low with the London physicians, being probably classed among their "acute infantile."—See on No. 22. That the mortality should have been higher here than at Edinburgh, I can only refer to the late prevalence of hooping-cough in this town.

2. *c.* On comparing this section with the former in the years 1809–12 at London, it is manifest that the vast ratio of convulsions in the bills is not made up by cases of water in the brain being inserted there, as it has been inferred from the general decline in the former article, and the increase of the latter; since in the above period in *both species* there is a simultaneous rise. Is there not herein a sad monument of the privation and turbulent excitement during the desolating continental war? And the same character is remarkable in nearly all the diseases of that period.

3. The ratios of the London bills and of the London physicians concur in demonstrating the mournful pre-eminence of Plymouth in the mortality of this class. The low rate in India well shows the influence of climate. According to Krafft pleurisy kills *a third* of the inhabitants of St Petersburgh, and phthisis a sixth. In *Stockholm*, says Miander, the mortality of this class is *five times* the ratio of the country generally,

3. *a.* At Paris in 1823, phthisis formed a fifth of the general mortality, and pulmonary catarrh carried off 1 in 12. Dr Wells has observed, that Dr Woollcombe, in comparing the

frequency of the deaths from consumption in London, as entered in the bills, with those in Plymouth observed by himself, compares two very different things; and shrewdly conjectures, (from premises, however, which Dr Woollcombe, in a MS. note on this paper, states himself to be at a loss to perceive,) *that a greater proportion of deaths occur in Plymouth from pulmonary consumption than in London.* Dr Woollcombe again opposes Dr Wells, and attempts to confirm his own remarks by the authority of Dr Willan, who states the proportion of deaths from phthisis to the whole mortality in his practice for 1795 and 1796, to be 1 : 3.2, *i. e.* 77 of 241; while in Plymouth it was 1 : 4.3. It is singular that so accurate a writer as Dr Woollcombe should not have remarked that Willan, in his list of mortal diseases, places only one death under pneumonia, with six from cough after measles, and five from hæmoptoe. In fine, that few or no deaths are set down to answer to the vast number of cases termed in his register of *diseases, cough* and *dyspnœa.* So that with Willan the 77 deaths under *phthisis* comprise nearly all the fatal cases of chronic pulmonary disorders. Whereas Dr Woollcombe sets down 16 deaths under *dyspnœa,* which he leaves out of his collation with Willan's mortality. If we combine all the fatal analogous pulmonic diseases with Willan and Woollcombe, we shall find the ratios to the general mortality to be, for Plymouth 1 : 3.5, and in London (*i. e.* Cary St.) 1 : 3.25, which evinces a higher rate of mortality here, and verifies the conjecture of Dr Wells.

This fact, however, does not degrade the salubrity of this part of Devonshire for those consumptive invalids of the higher orders who come here from other places; inasmuch as both the prevalency and mortality of phthisis here is as much, if not more, affected by the physical circumstances of the people than by the climate. The practice of lace-working at the tambour, so prevalent among young females here, is a prolific source of consumption. Dr Bateman remarks, that "consumption in the London bills contains a great number of chronic diseases besides true *phthisis pulmonalis,* such as marasmus, mesenteric affections, sequelæ of hooping-cough, small-pox, &c. in children, with diseases of the viscera in adults." Yet the ratio of consumption in the bills for 1795–1800 to the aggregate mortality is 1 : 3.8; and with Willan at the same period it is 1 : 3.3, (*i. e.* more than in the bills,) and with Woollcombe 1 : 4.2. This is a mathematical argument of Bateman's error. Let those who hold in contempt the London bills explain the fact of this curious correspondence.

As all ulceration is a sign of a bad constitution,—the result of the habits of civilized life,—the proportion of this genus would indicate the vigorous habits of the age, or the contrary. I have been favoured with a very interesting letter from Mr H. Montague Phillips, surgeon at the Royal Cornwall Infirmary at Truro, on the comparative prevalency of particular diseases there, from which I beg to insert an extract.—" My professional brethren here unequivocally assert, that the most prevalent disease in this neighbourhood is *phthisis pulmonalis*, which is the fatal termination of a great majority of their cases, even to the incredible number of 75 or 80 in every 100. And this may in some measure be attributable to our humid and variable climate, and the peculiar occupation of the bulk of the lower classes of society, *i. e.* excavating the bowels of the earth, inhaling impure and mephitic air. Rarely do we meet with a miner who has attained his fiftieth year not labouring under some affection of the chest which brings him to a premature grave."

4. Should not the large proportion of this genus at Paris be ascribed to the popular diagnosis introduced by Corvisart?

5. Here again is an instance of the peculiarity of diagnosis which now prevails at Paris.

6. The proportion at Manchester below ten years of age, the period of life which is most liable to these maladies, is far smaller than the physical condition of its inhabitants would lead us to expect. Is there not some error here? This section contains a sad monument of the sufferings of the British Army in its retreat from Spain.

7. Willan's enormous ratio is an evidence of the debauched habits of the lower classes in London.

9. This class seems to be very prevalent at Plymouth, which shows that cold with moisture is the chief cause of urinary complaints. The habits of affluent life are shown by Morgan's ratio to be highly injurious to the urinary organs.

10. Is not the recent increase of deaths in childbed to be inferred to the greater frequency of abdominal inflammation?

11. The fact asserted in the London bills of the vast rise in the mortality of affections of the throat appears to be confirmed by the observations of physicians. What can explain its singularly high rate at Exeter? The immense disparity in the mortality from croup or Stopping at Glasgow, as given by Dr Watt, and at this place is very remarkable. There it is stated to form a twenty-fifth of the entire mortality, and here, during the same period, only 1 in 321 deaths.

14. From the writer's observation, which accords with the proportion of this class at Edinburgh, erysipelas is often fatal there by a transference of disease to the brain.

16. The absolute increase in the deaths from measles during

late years is sufficiently established by the ratios both at London and Plymouth. At Glasgow, at the beginning of this century, it formed a 34th of the whole mortality, and here, as given by Woollcombe, a 54th. Heberden thinks that eruptions occasioned by the scarlet fever may have been mistaken for measles. The mortality from this malady at Manchester is enormous. I may here remark, that, in order to make Roberton's abstract from the bill of mortality in that town in persons below ten years of age comparable with the other registers in these tables, (with regard only, however, to such diseases as chiefly happen during this period of life,) I take the ratio of his proportion of particular diseases to *double* the total mortality assigned by that gentleman, as I find that at Plymouth nearly *one-half* of all the deceased were below ten years.

17. That the small pox in London should now constitute nearly one-half of its mortality before the use of vaccination, and be *five times* more mortal there than at Paris, is a matter of national lament. Although it is still fearfully high at Plymouth, it has fallen in a greater ratio here than in London. What has extinguished it at Clifton? How singular, that, at the Edinburgh New Town Dispensary in 1820–23, it should actually surpass the proportion in Willan's practice in 1795–96, and 98. With Dr Donald Monro, in the English Army at Coxheath in 1779, of 63 total deaths, 14 were from small-pox. Dr Caspar states that at Berlin from 1782 to 1791, 4453 deaths happened from small-pox; from 1812 to 1822 only 555, and the proportion to the general mortality was 1 : 1635. In 1789 the deaths in infants by this malady were 1 in 4 : in 1820–22, only 1 in 785.

Caspar also states, that before vaccination in Berlin, from 1786 to 89, 39 children in 100 of the population died by their diseases; but in 1819–22 only 34 of 100. This accords with the ratios of mortality and population at other places for recent years, but by no means refutes the argument of Dr Watt, who, I think, has been censured too severely from an oversight of the distinction between the *comparative frequency and mortality of particular diseases inter se*, during any given period, and the consideration of *disease* and *death* in *reference to the existing population.* He demonstrated, not inferred, that the mortality of children in Glasgow had not varied in two equal periods before and after the introduction of vaccination: and, in reference to the total mortality in each period, it certainly had not fallen. And be it observed, that, as the periods compared were equal, the disparity of the total mortality was of no consequence to his argument.

His observations are comparative only, regarding infantile deaths in their ratio with the adult mortality. The total deaths in respect of the population had greatly declined in the later period. Was then the decline equal among infants and adults? or was it greater in the infants, as might be expected from the use of vaccination? This was to be fairly determined by comparing the proportion of deceased infants to the adults in certain equal periods. The result was, that, while fewer infants had perished in the later period out of a given number of the existing inhabitants, and the general mortality had also declined, *the decline was greater on the part of the adult* than of the infant, in whom certain diseases had risen in mortality to take the place of the small-pox. The fact, however, which Watt notices of the vast amount of those infantile disorders is peculiar to Glasgow; and, as I have shown in the Transactions of the Plymouth Institution, is not seen in the recent London bills of mortality.

18. At Glasgow, from 1806–1812, it constituted a twentieth of the entire mortality.

19. At Glasgow, from 1806–12, fever made 6.49 per cent. on the aggregate mortality; in the London bills at the same period about 7 per cent. With Dr D. Monro, at Coxheath in 1779, of 63 deaths from all diseases 41 died of fever. Here, than, we may apply Heberden's remark on the decline of the plague in this island,—our change of manners, our love of cleanliness and ventilation, have produced amongst us a great inaptness any longer to receive it. At Paris in 1818, 962 males died of fever, and 1186 females, making 2148 out of 19455, the entire morality, or 1 : 9. Krafft says, that at Petersburgh fever destroys a third of the inhabitants.

20. *a.* and *b.* That more persons now die of inflammation and abscess, and fewer with mortification than formerly, is, I conceive, a proof that greater attention is paid to the diseases of the poor of late years, and greater skill and vigour exerted in their treatment, as well as that all classes have more comforts in food and clothing than thirty years ago.

21. In the bills at the early period this term comprised dropsy of the chest as well as of the belly and general body. The small variation in its amount during those three distant periods unhappily sets aside Dr Heberden's inference as to the improved habits of the lower classes, in reference to a less extensive abuse of distilled liquors.

22. This decline in the deaths by teething, as stated in the bills, accords with the known reduction in the mortality of infants of late years. But as the proportion here is so much inferior to Willan's ratio, and to that at Paris and Manchester, I suspect that some cases which would formerly have been

placed under this term in the bills are now transferred to the head convulsions,—as, conversely, Willan has certainly placed many deaths by convulsion under acute infantile. It is requisite, then, to combine the numbers here, and under convulsions and water in the brain, in the several periods, in order to ascertain the correct variations in these infantile maladies. The combined amount in the latter period is at London 1 : 6, and at Plymouth 1 : 10, and at Manchester 1 : 7.4, which is just what the different circumstances of the population in these places would lead us to anticipate, and is confirmatory of the accuracy of the bills.

23. The bills comprise, I presume, only the adult cases. Heberden thinks the recent decline of scrofula in them to be apparent and not real. The Plymouth ratios do not warrant the inference, that this malady has so largely declined here, as many cases of external scrofula are omitted in the recent amount.

25. The disparity in the proportion in the bills and with Blane, is explained by the vast number of this class who die in the poor-houses of the metropolis, and come not under the notice of physicians. The similarity in the ratios for London and Plymouth is another proof of the accuracy of the bills, as the burial register shows that more die aged here than in London.

TABLE III.—Exhibiting the Relative Mortality of Diseases in the aggregate at different places.

	Sick.	Deaths.	Relative Mort. 1 to
Combined Recent Plymouth,	11,416	552	20.5
Plym. poor-house	2700	127	21.2
Do. Dispensary,	3561	203	17.5
Author,	5155	222	23
Do. in 9 years at Edin. and Ply.	5621	271	20
Woollcombe, P. Disp.	4049	321	13.7
Exeter Disp.	2314	99	23.3
Devonport, ditto,	2385	96	24.7
Clifton, ditto,	4866	211	23
Chisholm,	1699	101	17
Howell,	3167	110	29.8
Blane, Hospital,	3835	374	10
Do. private,	3816	382	10
Glasg. Infirmary,	1057	52	20.3
Nottingham Hosp.	7850	472	16.6
Norwich Disp.	611	29	21
Convict ship,	157	1	
Plym. N. Hosp. 1806–9,	1984	368	5.4
Edin. Disp. 1821–24,	18,809	572	32.6

	Sick.	Deaths.	Relative Mort. 1 to
St. Thomas' Hosp. 1827,	11,615	259	45
Bartholomews, do.	11,447	350	33
St George's, 1825–27,			9
Bath Hospital, phys. in-patien.	280	14	20
Do. surgical.	271	16	17
Dub. hosp. 1822,	11,481	871	13
Hoxton Lunatic Asylum, 1809,	177	9	20
Do. 1810,	193	20	9.6
Do. 1811,	203	16	12.7
Do. 1812,	218	18	12
Do. 1813,	237	13	18
St. Lucia Gen. Hosp. 1781,	1339	731	2
Do. regiment,	5359	908	6
Indian army,	5519	423	13
Paris hosps. 1822,			8
Hotel Dieu,			6.8
La Charité,			5.5
St. Louis,			14
Lyons, Hotel Dieu			11

	Sick.	Deaths.	Relative Mort. 1 to
Montpellier,			10
Pesth,			6
Dresden,			7
Munich,			9
Prussia, garrison hospitals, 1822			78
Heidelburgh Hosp.			21
Russia,			9
Petersburgh,			4.5
Pays de Voud,			15.5
Geneva,			11
Brussels, adults,			9
Do. children,			6
Amsterdam, 20 years,			8
Genoa,			6
Milan, 1814,			6
Do. 1823,			7
Pavia, do.			10
Bologna, Tommassini's clinic,	453	35	13
Leghorn,			7
Palermo,			8
Spain,	29,730	2370	12.5
Valencia, civil,	4800	639	7.4
Do. military,	890	27	33
Barcelona,	8205	1293	6.4
Berlin, 20 years,			6
Vienna,			6 or 9.7
Copenhagen,			10
Walcheren,			
Sept. 10,	6931	221	31
17,	8141	277	29.7
24,	8754	287	30
Oct. 1,	9172	254	36
Haslar Naval Hosp. in 1782,	9130	513	17.7
1794,	8949	496	18
1799,	15,141	807	18.7
1804,	1667	140	11.8
1813,	3592	212	17
Hoxton Asylum in 5 years,	526	76	7
Plymouth N. H. in 1782	4784	136	35.2
1794	4237	164	26
1799	6799	174	39

		Sick.	Deaths.	Relative Mort. 1 to
Plym. N. H.	1804	3888	282	13.8
	1813	3563	231	15.4
Small Ports in	1782	9022	447	20.4
	1794	6062	162	37.4
	1799	2286	183	12.5
	1804	2095	203	10.3
	1813	2578	243	10.6
Home Station in	1782	22909	1096	20.9
	1794	19248	882	23.4
	1799	24226	1164	20.8
	1804	7650	625	12.2
	1813	9733	686	14
West Indies in	1782	5104	753	6.7
	1794	733	58	12.6
	1799	3846	467	8.2
	1804	3215	825	
	1813	2392	179	13.3
East Indies in	1782	2810	337	8.3
	1794	254	13	19
	1804	932	105	9
	1813	462	72	6.4
Mediterranean in	1782	794	36	22
	1794	1138	97	11.7
	1799	520	27	19
	1804	181	51	3.5
	1813	484	40	12
Total in	1782	31617	2222	14.2
	1794	21373	990	21.6
	1799	28592	1658	17
	1804	11978	1606	7.5
	1813	13071	977	13.3
Hamburg Hosp. in	1823	2660	386	7
		*1660	386	4.3
	1824	3351	350	9.6
		*2318	350	6.6
	1825	3431	378	9
		*2372	378	6.3
	1826	4116	489	8.4
		*3006	489	6.1
	1827	*3205	445	7.2
		4440	445	10

Comments.

As there are no circumstances which could have greatly changed the class of patients at the Plymouth Dispensary since its establishment at the end of the last century, I am entitled to conclude, that the variation above exhibited is a faithful index of the improvement in the situation of the sick poor here of late years. The decline observable in the relative mortality, while it shows that the poor now pay earlier attention to their maladies, is chiefly to be referred to the check recently placed on many of their most dangerous diseases. I have to acknowledge the urbanity of my excellent friend, Dr Thomas, physician to the Devonport Dispensary, for the examination of his Register, while I express my suspicion that many of the deaths are not ascertained at that institution.

Clifton appears to be an insalubrious place for the invalid; for its diseases seem to be generally severe; but I have no means of ascertaining whether it be, on the whole, an unhealthy place, *i. e.* whether the sick there bear a larger ratio to the valid population than elsewhere.

Sir G. Blane's hospital practice is of greater relative mortality than Willan's general practice, from its being the usage of the hospital to admit only the severer cases. The enormous proportion of deaths in his private practice shows either the malignant influence of the habits of fashionable life on disease, or that a London physician is consulted chiefly in severe cases.

If the Nottingham County Hospital admits a similar class of patients as the London Hospital, the deadly effect of the atmosphere and habits of the metropolis is impressively evinced by a comparison of the foregoing ratios.

The proportion of deaths is curiously similar in the London and continental hospitals.

The enormous disparity in the relative mortality at St George's with St Thomas' and Bartholomews' is explained by the returns of the former, comprising only the in-patients.

At St Lucia, the lowest ratio of the monthly relative mortality was 1 in 9 (in May,) and the highest 1 in 2.6 (in September;) but on deducting from the monthly returns of the actual sick the number of cases carried over from one month to the succeeding, the fearful fact is evinced, that one-half of the individuals sick in the course of a year perished.

It is a reflection that impresses me with awe, that every twentieth person of those who have sought the solace of medicine, under my own observation during the last nine years, has gone to the grave.

On the presumption that a larger proportion of severe cases become the subjects of public institutions, than in the miscellaneous practice of the profession in general, it may be inferred that about one in every twenty-five of the subjects of disease in this kingdom falls a victim to its fatal grasp.

The very small relative mortality observed when all the circumstances of the sick are under the control of a medical officer, and the habits of life regulated by discipline, as in the public service, and when disease is met in its earliest assault, is well exemplified in H. M. S. Britannia, and in the convict ship. Yet it is to be remembered that the persons here affected were previously in robust health, and in the vigour of adult life.

Insanity appears to be, on the whole, a mortal malady. The great variation in its relative mortality at the Hoxton Naval Asylum in different successive years deserves inquiry.

The mortality at Walcheren, when adjusted so as to be proper for comparison with St Lucia, &c. would be vastly higher than appears above; for, as I have taken the weekly returns of the sick and deaths, many of the cases sick on September 10th must have been carried on through the next two or three weeks, and may then have ended fatally. Probably, therefore, the total amount of individual cases of fever during the four weeks here stated did not surpass 10,000; of whom, in the course of the month, 1039 died, *i. e.* not quite 1 in 10, which is the ratio of the lowest monthly mortality at St Lucia. It is remarkable that the relative mortality in the navy on the Mediterranean station is similar to that of the Plymouth Dispensary about the same period.

Why was the mortality so much higher at Haslar than at Plymouth in the four periods before 1813?

The high mortality at all the naval stations in 1804 is a memorable index of the land service of the British fleet at that time.

By far the most fatal of our naval stations is the West Indian. Yet at the close of the war the mortality was higher in the small ports at home, and in the Mediterranean, than there. This may have arisen from the greater number of the wounded than in the European fleet. Sir G. Blane has, however, shown that during war more die in the navy of diseases than of battle.

Notwithstanding the recent improvement in the discipline of the army and navy, and the better skill of the medical officers in the service, the relative mortality at 1813 is seen to be higher than it was thirty years before.

I suspect that in many of the instances in this table, particularly in the returns from the public service, which were published by Sir G. Blane, the ratios are not the absolute measure of the mortality of the current maladies of each year; inasmuch as many of the 9130 sick at Haslar in 1782 may have remained in the hospital during a part of the following year, and then died.

The lines which are starred (*) in the Hamburgh returns exhibit the number of sick under treatment in each year, minus the number remaining at the end of the year, in order to show the absolute severity of the current diseases in those several years.

TABLE IV.—Diseases at Edinburgh New Town Dispensary.

	Autumn.		Winter.		Spring.		Summer.	
	Sick.	Deaths.	Sick.	Deaths.	Sick.	Deaths.	Sick.	Deaths.
Apoplexia and Paralysis,	16	4	11	4	17	4	2	1
Carditis,	3	3	2	2	14	5	4	3
Cholera and Diarrhœa,	82	2	106	4	116	4	220	8
Enteritis and Peritonitis,	33	8	18	5	20	8	24	6
Erysipelas,			37	4	35	3	21	1
Fever,	77	2	83	5	74	2	89	5
Do. Grattan, Dublin, 1818—19,	6978	347	5773	356	5320	213	7268	*224
Hydrops,	53	8	32	8	49	6	11	1
Hydrothorax,	8	2	22	14	15	4	5	2
Pulmonic Inflammation and Catarrh,	391	13	829	20	685	21	157	8
Phthisis and Hæmoptoe,	100	21	101	27	110	21	118	24
Pertussis,	33	8	54	8	18	4	14	2
Phrenitis Hydrocephalica, Convulsio,	24	15	24	18	31	24	27	17
Rubeola,	35	6	1	1	62	2	338	20
Scrofula,	14	2	15	7	8	2	36	5
Scarlatina,	42	6	43	4	61	3	17	1
Variola,	42	10	45	15	28	7	50	13
Diseases of Brain,	34	20	41	23	34	27	44	21
——— Lungs,	543	41	993	71	868	59	292	40
——— Liver,	31	3	25	6	17	3	31	4
——— Intestines,	119	12	139	16	153	15	256	18
Infantile acute,	14	1	83	2	41	2	13	2

By combining certain parts of the very valuable document of Dr Moncrieff on the diseases incident at the New Town Dispensary at Edinburgh for 1821–24, I am enabled to exhibit the influence of the different seasons on the relative mortality of particular diseases; and were a similar table to be prepared on a grand scale, from authentic materials, I believe that our prognosis of diseases would be rendered far more easy and exact than at present. The following table, it is supposed, comprises only the more serious and acute cases of disease. Possibly my estimable friend above-named may be induced to offer such explanatory comments as he alone, from observation of the cases, can supply.

TABLE V.—Exhibiting the relative mortality of Disease in the aggregate in the different seasons of 1826—27—28, derived from the practice of the author, and of the Public Dispensary.

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* During the whole epidemic from September 1817 to May 1819, in the first autumn the relative mortality was 1 in 16, and in the last 1 in 22; in the first winter 1 in 15, in the last 1 in 19; in the first spring 1 in 24, in the last 1 in 18; and in the summer 1 in 32.

	Young.		Adults.		Males.		Females.		Total.		Relative Mortality in the Y.	A.	M.	F.	Ratio to Gen. Mort.
	S.	D.	S.	D.	S.	D.	S.	D.	S.	D.	1 to	1 to	1 to	1 to	1 to
Spring.	515	22	1176	43	649	33	1042	32	1691	65	23.4	27.3	19.6	32.5	26
Winter.	424	15	1130	40	596	16	908	39	1554	55	28	28	37	33.3	28.2
Summer.	424	25	1079	22	571	24	932	23	1503	47	17	49	24	40.5	32
Autumn.	409	16	1030	28	494	18	946	26	1440	44	25.5	37	27.4	36.3	32.7
Total in the four seasons.	*1679	78	4139	133	2242	91	3442	120	5734	211	21.5	31	24.6	28.6	27

Comments on Table V.

In all the classes of subjects defined in the foregoing sections, the spring is seen to have been the most unhealthy season, and autumn the least so, except in the female sex of all ages, for whom the *winter* was the least insalubrious.

The most relatively mortal season, or that in which diseases are the most dangerous, or only the more serious sort of disorders prevailed in the *young*, is the summer; the least so the winter. In the *adult*, the most mortal, the spring; the least so the summer. In the *male*, the most mortal, spring; the least so, winter. In the *female*, most mortal, winter; the least, summer. In all ages of both sexes, the most mortal, the spring; the least so, summer.

Dr Bateman, in his historical survey of the diseases of London, states the comparative salubrity of the seasons on an estimate of 26,000 cases, during a period of twelve years ending 1816, to stand thus:—Autumn 524, summer 555, winter 569, spring 572. And in the present work the seasons exhibit the same order of comparative *sickliness* for three recent years; but this order of comparative *mortality* for those years is spring, summer, winter, and autumn. This estimable author further remarks, that the change which the seasons have undergone in their comparative salubrity consists not in the transference of disease from one season to another; not in the multiplication of the maladies of the *saluberrimum Ver*, and the subtraction of the dangers of autumn, but in the removal of the evils of the formerly unhealthy seasons, without the addition of new sources of mischief to the others. I greatly fear that this agreeable view of the salubrity of the present age is to be pourtrayed in sadder colours from the deduction in the second table of this essay.

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* These totals of the sick are less than the sum of the above sections, inasmuch as the dispensary *readmissions* of the same case of disease are excluded; consequently, the ratios in the four upper sections are rather too low as an estimate of the relative mortality of *distinct cases* of disease in the several seasons: but this does not affect the value of the ratios for one season as compared with those of another season in this table.

The proportion of deaths to the total of the sick is seen in the above table to be in the male sex as 1 in 24.6, and in the female as 1 in 28.9. In the Dublin Hospitals in 1826–27 there were 4458 sick males, with 435 deaths, and 7023 females, with 436 deaths, which accords with the principle illustrated in my former comments. So also at the Glasgow Infirmary the relative mortality in the males was 1 in 8.7, and in the females 1 in 10.2.

At Amsterdam likewise the rate of deaths in the male was as 1 in 7, and in the female 1 in 9. But at Milan in 1823 the proportion of mortality was higher in the female, there being in her 888 deaths of 6964 sick, and in the male only 1582 in 22,762 cases of disease. In the Hotel Dieu at Paris also the ratio of mortality was in the female 1 in 5, and in the male 1 in 7. In Prussia more females than males die of old age, as 1080 to 950; and more females die of acute diseases, but more males of sudden death. The deaths among women in childbed to the general female mortality are as 273 to 10,000.

The ratio exhibited in this essay may be considered satisfactorily to demonstrate the admirable law of compensation adverted to in the April Journal, by which the Divine Providence adjusts the incidence of disease and death in the different seasons on the various classes of mankind; and the evidence herein presented that the events which may have seemed to be the most chance-like and unequal, are a part of the all-comprehending scheme of the Divine government, should serve to inculcate on the sufferer submission and trust in the righteous Arbiter of man's destinies.

TABLE VI.—Exhibiting the comparative Mortality of the Seasons; derived from the Burial Registers of St Andrews and Charles, Plymouth, and the parishes of Plymstock and Plympton Mary, for 1823—28, inclusive.

Period.	Infants below 2 years.	Young, *i. e.* below 15 years.	Adults. Male.	Adults. Female.	Adults. Both Sexes.	Total.
Spring.						
1823	65	98	36	39	75	173
1824	48	63	31	33	64	127
1825	64	115	28	29	57	172
1826	25	37	36	34	70	107
1827	34	48	44	43	87	135
1828	72	93	29	40	69	162
	308	454	204	218	422	876

Summer.							
	1823	37	64	20	29	49	113
	1824	97	142	30	16	48	188
	1825	68	114	33	35	68	183
	1826	41	54	38	31	69	123
	1827	25	32	27	25	52	84
	1828	67	98	40	29	69	167
		335	504	188	165	353	857
Autumn.							
	1823	42	55	26	36	62	117
	1824	34	55	29	30	59	114
	1825	40	66	42	33	75	141
	1826	28	43	34	44	78	121
	1827	51	63	23	33	56	119
	1828	43	71	43	33	75	146
		238	353	197	209	406	759
Winter.							
	1823	34	43	33	38	71	114
	1824	53	88	37	35	72	160
	1825	45	61	40	39	79	140
	1826	22	33	36	38	74	107
	1827	48	73	39	38	77	150
	1828	46	69	62	48	110	179
		248	367	247	236	483	841
Total Plymouth in the above six years.		1129 R. F. 1 : 3	1678	836	828	1664 R. F. 1 : 2	3336
Plymstock,		127	189	116	109	225	414
Plympton Mary,		79	121	110	100	210	331
Country,		206 R. F. 1 : 3.6	310	226	209	435 R. F. 1 : 1.7	745 745
Grand Total,		1335	1988	1062	1037	2099	4081

TABLE VII.—Showing the variation in the annual Mortality in the Town and Country for the former period.

	1823.	1824.	1825.	1826.	1827.	1828.
Plymstock,	55	60	58	38	40	38
Plympton Mary,	44	46	52	29	35	28
Country,	99	106	119.	67	75	64
Plymouth,	515	588	636	448	488	654
Total,	614	694	746	515	563	718

This very important table would have become still more interesting could I have obtained an estimate of the analogous classes of the existing population during those years. In its present state, however, many useful suggestions may be derived from collating it with the former table, which is indeed a better criterion of the incidence of *disease* than of the actual deaths; as probably in many instances we failed to ascertain the mortal result.

The burial register is an infallible mode of supplying this defect. Moreover, a collation of the burial registers in various places, apart from any statistics of diseases or of the population, would lead to a variety of useful information.

Summer is seen above to have been the highest and autumn the lowest in mortality among the young; winter the highest and summer the lowest in the male and female adult. The spring was on the whole the most mortal season, and autumn the least so, during those six years; and in the column of totals in the former table, the former season is also seen to have been the most, and the latter the least sickly.

The latter table remarkably accords with Dr Heberden's deductions from the bills of the last century, that of the aged the greatest number die in the coldest months, and the fewest in the summer.

The order of the *months* in relative mortality at Plymouth during four years, 1823–27, is thus:—June, 218; May, 204; August, 189; July, 188; March, 186; February, 185; January, 181; April, 174; September, 173; October, 167; December, 156; November, 150. Total in the warm six months, 1148; in the cold 1044. This approaches to the description which Celsus has given; "Saluberrimum Ver est; proxime deinde ab hoc hiems; periculosior æstas; autumnus longe periculosissimus." This order, however, is curiously different from that observed by all the former English writers on this subject,—Short, Percival, Price, Currie, Woollcombe, and Bateman. The admirable meteorological work which my friend, William Snow Harris, has been sedulously preparing for some years past, will, I hope, illustrate this strange and important fact.

How greatly the habits and atmosphere of a large town affect the comparative mortality in youth and mature age, is shown by the ratio of deaths in infants at Plymouth, being to the aggregate mortality as 1 in 3; and that for the neighbouring country parishes being only 1 in 3.6. Plymstock is a few miles nearer to Plymouth than Plympton Mary, and the mortality of infants in the former place bears a larger proportion to the aggregate than at the latter. In this town *not one-half* of all the dead had passed the age of puberty. In the country far more than a half were adults. In an abstract of the burials at Edinburgh in 1810, given in Vol. vii. of this Journal, by the late venerated Dr Duncan, there are children 1363,—adults 1584; the latter forming more than half of the whole mortality. As the precise age to which the term children is applied is not stated, I cannot strictly compare the relative mortality of these classes here and there. The very high mortality in children at Leith in comparison of Edinburgh is truly shocking, and its causes should be well sought out. The proportion surpasses all those in after life, as 278 is to 204. The same predominance is found in the register at St Cuthbert's chapel alone of all the Edinburgh kirks, the number of children to the adults being there as 140 to 113. Presuming that children is meant above to express all below ten years, the ratio in them to the general mortality at Edinburgh is 1 to 2.16; in London for the same period, 1 to 2.17, *i. e.* rather lower; and at Plymouth 1 to 2.27, which is still less. At New York in 1827 the deaths below two years were 1882 to 5181 general mortality, or 1 to 2.7; and those below ten years 2456, or 1 to 2.11.

It is curious that in all the registers in my tables more males than females are found to have died, and I suspect that this is a universal rule. So likewise more males and females are to be found in the baptismal registers. Herein is another proof of a superintending Providence.

In a paper which the Plymouth Institution have done me the honour to publish in their Transactions, I have constructed a table from the burial register of this town of the mortality at *different ages* in *both sexes*, during four recent years; from which it appears that 103 more male than female infants died in that period; but that after the age of fifty, forty-six more females than males were interred. In the mean periods of life, the number deceased of both sexes was nearly equal. And the disparity in the male and female infant deaths happened only in the years 1823–24, and 1824–25, when the measles and small-pox prevailed, which latter disease is shown in the first table of this sequel to be far more mortal in the male sex. This curious corroboration of these statistics shows that the subject merits ample attention. The difference in the male and female infants deceased in *each* of the four years above referred to is,

in 1823–24,	104 Males.	74 Females.
1824–25	147	95
1825–26	116	101
1826–27	66	50

In 1824 and 1825 it is to be remarked, that the mortality was very high both in Plymouth and the adjoining country. In the former year the measles were epidemic, and in the latter the small-pox. And this dreadful malady is seen even thirty years after the establishment of the virtues of vaccination to bear the cypress of paramount mortality over the former, which has itself so greatly risen in late years. At Glasgow, on the contrary, from 1806–12, Dr Watt states the mortality from measles to be ten per cent. on the aggregate mortality, and that from the small-pox to be only four per cent., while in the London bills about the same period the mortality from *measles* was only four per cent. In 1828 the mortality at Plymouth was the highest in the entire period, while in the country it was the lowest; that severe pressure of death happening in the winter, which season is lenient in the young, and falling on the adult. Whence it may be inferred that the country is milder and far less mortal in a severe winter than the town.

The tables and observations contained in this essay will serve, it is presumed, to show that medical statistics open a wide and fruitful field of inquiry. As they certainly increase in individual inquirers the fascination of his profession, they tend to fix his study on the remote causes of diseases;* and, above all, they elucidate the grand general laws by which the most fluctuating of all subjects are controlled and administered.

At a future period I hope to subjoin some practical commentaries on the more important diseases incident at Plymouth.

ART. II.—*Quarterly Report of the Edinburgh Surgical Hospital from August to November* 1829. By JAMES SYME, Esq. Fellow of the Royal College of Surgeons, and Lecturer on Surgery.

SINCE the date of last Report, the cases of surgical disease, for which relief has been sought at the hospital, have increased in number and importance.

553 patients have applied within the last three months. Of these 64 have been admitted into the house, and the remainder have been treated as out-patients, receiving advice and medicines from the institution.

It was my intention to have written a report of the more interesting cases which have occurred within the period abovementioned; but I find that my Systematic and Clinical Lectures, together with my professional avocations in private, do not afford me the requisite leisure for this purpose at present. I will therefore, in the meantime, merely make some observations on the treatment of ulcers, and at a more convenient opportunity take a review of the other cases.

The very great number and variety of ulcers which have been presented at the hospital, afforded me ample opportunity of illustrating the treatment of these most frequent and distressing affections. I felt much satisfaction in doing so on several accounts. 1*st*, The great relief derived by the patients, many of whom were restored to the means of earning a livelihood, of which they had previously been long deprived. 2*d*, The useful instruction derived by my pupils, who were wise enough to regard the ordinary duties of their profession as deserving of at least as much attention as those great and bloody exploits, which occur but seldom in the course of practice. 3*d*, The widely extended and sound reputation acquired by the new Institution from curing diseases, where the relief is so great and manifest, and the means of remedy are so little disagreeable.

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* My sense of the importance of this cannot be better conveyed than in the words of Gaubius;—"Harum ideo quam utilis et necessaria sit inquisitio patet. Quodsi vetus empiricorum schola in his solis malé acquievit, *magis etiam culpandi sunt recentiores* qui, remotis *insuper* habitis, *proximæ* tantum scrutinio sese addicunt."

It would be out of place for me to enter here into a detail of cases which are usually reputed trifling and uninteresting, though there are few surgical diseases which more seriously affect the patient's comfort; but I cannot refrain from reporting at some length, and with some care, the advantages of a practice which I have lately introduced in their treatment, and which I believe is a new one.

In treating what are called Indolent ulcers of the leg, I used to regard the plan recommended by Mr Baynton as approaching nearly to perfection, and still believe that, when properly executed, it will sooner or later effect a cure, if a cure be practicable; but another method has lately suggested itself to me, which seems in many respects preferable.

It is not unusual to meet with cases of indolent ulcers, which after exhibiting their characteristic obstinacy in opposition to the most careful treatment, heal up at once without any attention, so soon as the limb begins to recover from an attack of phlegmonous erysipelas which it has happened to suffer. The observation of such cases led me to try the effect of inducing a similar inflammation artificially, and the result has fully equalled my expectations. The means employed for this purpose were blisters, and the object being to excite a smart and diffuse inflammation, they were not limited in extent to the size of the sore, but were made to cover a great part of the limb, and were allowed to remain in operation for a long while, sometimes even twenty-four hours.

The first effect of the blisters in these cases is a more than ordinary inflammation and discharge, the surface sometimes continuing to suppurate profusely for several days, just as if the cutis had been denuded by a scald or burn.

In a day or two the patient is agreeably surprised by observing that the œdematous swelling of the limb, which so constantly accompanies ulcers of the kind under consideration, begins to subside, and in the course of a very short time, rarely exceeding a week or two, it nearly or entirely disappears. The consequence of this detumescence is a great diminution in the size of the sore, which also comes to be on a level with the surrounding skin. Then the surface takes on a healthy granulating appearance, and the sore heals, partly by contraction, partly by the formation of a cicatrix. For the first few days after the blister has been applied, some simple ointment may be used, just as in the ordinary treatment of a blistered surface, and afterwards a wash of acetate of lead or sulphate of zinc, in the proportion of one or two grains to the ounce. If the sore should again prove obstinate, the blister may be repeated, and if a small part remains stationary towards the conclusion of the cure, it ought to be filled with the red oxide of mercury, or a mixture of this powder with flour. My friend, Professor Davidson of Aberdeen, induced me to try this application in the treatment of ulcers, and I cannot say too much in its praise, especially in the case just mentioned. After one or two dressings it forms a firm crust over the sore, which ought not to be disturbed, and renders any farther interference unnecessary.

I have no hesitation in ascribing the good effect of blisters which have been just described to their stimulating the absorbent vessels, so as to remove the œdema. We know that blisters possess a singular power of doing this, as is exemplified in the cure of dropsies of the joints and bursæ, and it is easy to see that the existence of œdema must be an insuperable obstacle to the healing of the ulcer. It prevents the contractile effect of the granulating action, and thus occasions a struggle, which probably gives rise to the pain and other symptoms that so often induce a resemblance between indolent and irritable ulcers. And we find, in fact, that all the modes of treating the ulcers in question which have ever proved serviceable, such as the horizontal posture, the roller of Underwood and Whateley, or the adhesive straps of Baynton, tend to reduce œdematous swelling. Some frivolous and wrong-headed improvers have advised that the straps should not encircle the whole limb, but only two-thirds of it, and in so far have done what they could to bring themselves and the practice into contempt. I lately cured an ulcer on the leg of a lady which had existed without interruption for twenty years, and was deemed incurable, because it had resisted the most assiduous exertions of several surgeons in town. When I proposed to apply adhesive plasters, the patient would hardly consent, because they had been tried previously without success. I ascertained that they had not encircled the limb, and hence the failure.

In order to appreciate fully the good effects which attend the removal of œdema, it is necessary to have clear ideas of the process that is employed in the healing of an ulcer; and as it seems to me that the truth in this respect has been, and still is, very much overlooked, I may perhaps be excused for making a few observations on the subject.

It is a common remark, that the things which lie most within our sphere of observation are least attended to, and there is no better illustration of this than the subject which I am now going to discuss. Every wound, when it does not unite by the first intention, and every ulcer, if it heals at all, heals by granulation, and yet nobody takes the trouble of inquiring into the precise nature of this process, every one being satisfied with the vague account of it which is to be found in surgical books. The statement generally contained in these is, that lymph being effused on the surface of an abscess, wound, or ulcer, becomes organized, and formed into a layer of small red, pointed fleshy granulations; from these pus, together with coagulable lymph is secreted, and the new crust of organizable matter is in its turn converted into granulations. "In this manner layer after layer is formed, until the cavity is filled up."* Such is the explanation usually or rather universally given, and as generally considered satisfactory.

Instead of attempting to disprove the accuracy of this description of the mode in which losses of substance are repaired, by criticising the process itself, I will at once endeavour to show, that the alleged reproduction does not really occur; that the appearances which are thought to establish its reality are delusive; and that, therefore, the explanation employed to account for it is equally unnecessary and erroneous.

The subject of Reproduction or Incarnation engaged the attention of the French Academy of Surgery; and the memoirs of its members, Fabre and Louis, leave little wanting with regard to its history. How they have happened to be so completely forgotten in modern times, as to permit the old doctrine of regeneration to revive and flourish to the suppression of the truth, I cannot tell, but shall attempt to place the matter once more in a clear point of view.

The arguments in favour of reproduction by granulation are, 1. the regeneration of lips, tongues, and the glans penis completely or partially destroyed; 2. the filling up of abscesses or sinuses; and 3. the healing of wounds attended with loss of substance.

When we come to inquire a little particularly into these proofs, we find, that of regenerated lip there is only one instance on record; and if M. Louis's account be correct, not much weight ought to be founded on it. Louis says, that M. Pibrac and he were invited to visit this famous lip, in order to satisfy themselves of its actual regeneration. The loss of substance, they were told, had been so great, that it was impossible to unite the cut edges; that it had even been necessary after the excision to apply the actual cautery to the roots of the disease; that eventually the patient was cured; that the lip was restored; and that they should be lost in admiration of this wonderful work of nature. "We visited the patient," says he, "and saw nothing at all extraordinary. The patient had been freed from a tumour, the extirpation of which might at first sight have seemed to require removal of the whole lower lip. But it is well known that when a part is much swelled, a large portion may be taken away without diminishing its natural extent; and this is just what had happened in the case under consideration. The lip, properly speaking, was completely wanting, so that the teeth and gum appeared through the breach. *The loss of substance had not been repaired.* A perpendicular cicatrix denoted that the lower part of the wound had been closed by the union of its edges. The patient was unable to retain his saliva, and the skin of the chin, which had been drawn up to supply the lip in part, and which had been taken for the product of regeneration, was covered with its characteristic *beard.* The great breach caused by the operation had been in some measure closed by contraction, but not one particle of the lost substance had been regenerated."

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* Sir Astley Cooper's Lectures, Vol. i. page 161.

If this be the true account of the lip, I certainly feel inclined to agree with M. Fabre, that the "well formed proportioned glans," which Mr Jamieson of Kelso had the pleasure of seeing reproduced after "amputating the Balanus, preputium, and a small portion of the corpora cavernosa penis," was nothing more out of the ordinary course of nature than the original glans itself, which had been concealed from sight by the swelled and gangrenous prepuce. If the alleged reproduction admits of this explanation, in regard to the lip and glans penis, it is evident that we should pay little attention to the evidence afforded by the separation of sloughs from the tongue, where it is so difficult to ascertain the extent of destruction and also of restoration, owing to the great extensibility of the tissue composing this organ.

As to the filling up of abscesses, it ought to be recollected that the cavity which contains the pus is not formed by the destruction of any tissue, but by distension of the skin and subjacent organs, whence evacuation of the fluid is followed immediately by such collapse and approximation of the surrounding parts as nearly obliterates the hollow. And where there is really a destruction of cellular substance, as in the abscesses of weakly children, a permanent depression remains after the cure is completed. Sinuses are healed on the same principle, viz.

by coalescence of the sides; and every attentive practitioner must have observed the great advantage of laying them together by means of proper bandages, so soon as they are disposed to unite.

Lastly, as to the healing of ulcers or wounds attended with loss of substance, it is well known that the resulting cicatrix is always smaller than the original sore; and that there is invariably a depression of it proportioned to the destruction of parts. M. Fabré quotes the case of a wound in the thigh destroying the muscle and exposing the bone, the cicatrix of which rested directly on the bone without the interposition of any newly formed substance. It was a similar case which led my attention to this subject. M. Bezoet of Rotterdam, who wrote against the doctrine of reproduction, appeals to a case of wound in the head, where a portion of the scalp was removed, and where, notwithstanding the most luxuriant granulations, the cicatrix rested on the bone. I have in my possession several skulls, with the scalps, from which portions of bone had been removed, artificially and naturally. One of these belonged to a gentleman who was attended by myself, and in all of them the cicatrix is but a pellicle resting on the bone or *dura mater*.

When amputation was performed in the old way by cutting directly through all the parts, a very large granulating surface resulted, which ought to have become larger if there really was a growth of new substance, so as to form the stump into a bulbous figure. But it always happened, on the contrary, that the cicatrix was much smaller than the wound, and that the stump, so far from being bulbous, was of a sugar-loaf or conical form.

We have frequently an opportunity of dissecting a granulating sore after death or amputation. And do we then find any such appearance as the common description would lead us to expect? Do we find successive layers of granulations, or any considerable thickening of newly formed substance? I have made many such examinations, and could never perceive any thing but a thin layer of organized matter covering the surface.

How then are wounds healed by granulation?

Lymph being effused over the surface and organized into a granular pellicle, lymph and serum are effused into the subjacent cellular tissue so as to distend it more or less. In what is called a healthy granulating ulcer, the quantity is very small, so as hardly to affect the elevation of the surface, or induration of subjacent parts, but when the process proceeds in a morbid manner, then many remarkable phenomena are thus induced.

For instance, when the ulcer is defective from weakness, the granular surface is distended by serum, effused under it, and elevated into soft spongy projections, the proud flesh of the vulgar. This œdematous state of the sore is usually accompanied by a similar condition of the limb or other part in which it is seated; no trace of these fungosities is to be found after death; pressure restrains them as effectually as it does the œdema of the limb; and they suddenly subside under the action of what are called escharotics without leaving any remains in the place they occupied. These observations afford satisfactory evidence of their real nature, which I had lately an opportunity of illustrating still more convincingly. William Brown entered the Hospital on the 27th of October to obtain relief for a most extensive and frightful sore of the leg. It had existed for seven years—it stretched from the middle of the calf, on the back and both sides of the limb, to the heel. There was great general swelling—enormous fungous growths—complete inability of motion—in short, such a formidable appearance as led the patient, and most of the medical men who saw him, to conclude that amputation afforded his only chance of remedy. A large blister was applied, and the state of matters soon suffered an extraordinary change for the better. The swelling of the leg subsided—the fungosities disappeared, and the sore, now greatly diminished in extent, assumed a healing appearance. At the end of two weeks from his admission, I desired him one morning to try if he could walk. When he had accomplished a few steps, I observed the blood trickling from the ulcerated surface; and, on observing it more closely, discovered a number of dark-coloured eminences which had not existed previously. They varied in size from that of a pea to that of a nut, and exactly resembled proud flesh—or fungosities of weak ulcers. When squeezed roughly they suddenly burst, and collapsed with effusion of blood. When pressed more gently, the pellicle forming them could be distinctly traced over the adjacent granulating surface, where a similar appearance was produced by the blood forced out of the little tumours formed in the first instance. It is difficult, I think, to conceive a more satisfactory proof of the opinion which has just been offered as to the nature of a granulating surface than this observation, which I am very much surprised should not have been made before.

In those sores, again, which have something specific in their action—something capable of contaminating other parts of the system, either by absorption or extension of action, there is an effusion of lymph into the subjacent and surrounding cellular substance, which occasions the indurated and elevated edge so characteristic of such sores.

In other ulcers, as those occurring in scirrhous and medullary sarcomatose textures, lymph is effused under the granulating surface, and becoming organized, elevates it into fungous excrescences. And here I dare say it will be asked, if morbid structures be reproduced, why cannot the healthy tissues be regenerated? To which I answer by asking, Since the legs of salamanders and lobsters are reproduced after removal; why are not the limbs of man also reproduced? In studying the operations of nature we ought always to prefer facts to reasoning.

The granular surface being formed, so as to seal up the interstices of the body and serve as a temporary integument, the process of healing proceeds by levelling the sore with the surrounding skin. The slight effusion which, as already mentioned, obtains under the granulations, assists in this, but the effect is chiefly produced by absorption, consequent emaciation, and sinking of the surrounding tissues, especially the fat. So soon as the surface of the ulcer, or the edge of it at least, is on a level with the skin, the absorbing process goes on more rapidly, removing the interstitial particles of the granular pellicle and subjacent tissue, and thus diminishing the area of the sore. In illustration of this part of the process I may remark, what every attentive surgeon must have observed, that the cicatrix of ulcers is smallest in those parts of the body where the integuments are most lax, as the scrotum, the abdomen, the mamma, and that it is largest where the skin is most unyielding from its firm connection, as the thorax, the cranium, the shin, &c.

The cicatrix is formed of the granular pellicle and subjacent cellular substance, which has been indurated by the effusion of lymph into its cells. Its thickness varies with circumstances, but always diminishes after the cure seems to be completed; whence, though when first formed upon a level with the surrounding skin, it ultimately becomes lower, so as to constitute a depression; but this appearance is no doubt partly owing to the surrounding parts recovering from the emaciation, which, as above-mentioned, takes place in the first instance as a step towards the cure.

Though there is no reproduction by granulation generally speaking, there is one exception in the case of bone, which we every day see regenerated after exfoliation, compound fracture, &c. This also may be argued in favour of the common opinion, but may be answered as above, that analogy is no proof against facts. Why bone should be formed by granulation, and the other tissues should not, I cannot tell. Nature is the best judge of her own proceedings. And our business is to study their results, not the motives which led to them.

The length to which this paper has extended, renders me averse to detailing any of the particular cases treated by blisters in the Hospital, as I intended to do. Suffice it to say, that various people who had suffered for many years,—one man for twenty-five years, had been treated unsuccessfully in other hospitals, and were considered incurable,—speedily recovered under this treatment.

75, George Street,
17th November 1829.

Art. III.—*A short Abstract of the Prize Essay of Dr William Horn of Berlin, on the Poison contained in German Sausages, &c.*[*] By R. Arrowsmith, M. D. Member Extraordinary and formerly President of the Royal Medical Society of Edinburgh.

THE subject of the prize essay,[†] which was published in Berlin during the last year by Dr William Horn, son of the celebrated Professor Horn of that city, was first proposed by the Berlin Medical Faculty in the year 1825. As, however, no essay was submitted to the faculty which was considered to be deserving of the prize, it was again proposed in 1826, and on that occasion the production of Dr William Horn was crowned by the reward.

In taking upon me to present some notice of it to British toxicologists, I know I am acting in conformity with the wishes of the zealous and intelligent author. Although I am not aware that any instances of poisoning have occurred in this country from poisonous *farcimina*, yet it is by no means impossible that such may occur; and at any rate it will not be uninteresting to toxicologists to learn the effects of a poisonous agent of this kind on the animal economy.

Instances of such poisoning have not been uncommon in Germany. Towards the end of the last century it was first discovered that "farcimina"[‡] occasionally contain a poisonous substance. And although the magistracy had recommended a greater degree of care to be observed in the preparation of these articles of food, farther instances of poisoning continued to occur. In the kingdom of Wurtemburg, where Dr Horn particularly mentions that farcimina are generally smoked, the greatest number had been met with: but neither there nor elsewhere had any accurate examinations been instituted to discover the nature of the poison. It was chiefly agreed that it was generated in the sausage. All intentional attempts to render farcimina uniformly poisonous by a particular mode of preparation have failed. Putrescence does not make them so; and in those farcimina which have proved poisonous, no odour or taste of a particular nature has been perceived.

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[*] De Veneno in Botulis scripsit Guls. Horn, Med. et Chir. Dr. Commentatio in certamine literario a gratioso Medicorum ordine Berolinensi Præmio ornata. Berolini, 1828.

[†] "Cujus indolis est venenum, quod in farciminibus corruptis gigni dicitur, et quinam ejus sunt effectus in corpore humano et animali in genere: qua de re quæ nota sunt, colligantur et accurata disquisitione examinentur, novaque addantur experimenta."

[‡] The following are the various kinds of "farcimina" alluded to in this work. 1. Schlackwurst, a sausage made with lean meat and spices in the large intestine. 2. Apexabones, or Blutwurst, black puddings. 3. Knackwurst, a kind of sausage made of pork, to which sometimes veal is added, and calf's blood. Aromatics, herbs, and spices are added, and sometimes milk, in which case they are eaten sooner. Others are made of pig's tongues; others of liver, or of the meat about the

Farcimina are no doubt carelessly prepared in many places; but the poisonous effects of them have been confined to a limited part of the country. In Brunswick, where they are composed of numerous ingredients, they are said to be always eaten fresh, and on this account never prove poisonous.

Kerner * and Weiss † are the principal authors on the subject. The former has referred to 155 cases which had happened in Wurtemburg between the years 1793 and 1822, and of these eighty-four died. Weiss has related in his work twenty-nine, to which Kerner has added eight more, all having occurred in the kingdom of Wurtemburg. The proportion of fatal cases among the latter is not mentioned.

Kopp, Kühn, Buchner, and others, have related a few additional examples.

The disease induced by poisoned farcimina when it has not proved mortal, has been almost always of long duration, continuing even for years, and the symptoms most slowly disappearing. When it terminates fatally, the patients die calmly and free from delirium, death, according to Kerner, taking place in the way of paralysis or asphyxia.

In about twenty-four hours after the poisonous food is taken, burning heat of the stomach comes on, accompanied by sanguinolent vomiting, and tormina of the bowels. Pa-

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pig's head. These are also submitted to pressure after boiling, and hence are called Presswürst, &c.

* Neue Beobachtungen über die in Würtemburg so häufig vorfallenden Vergiftungen durch den Genuss geräucherter Würste, von Dr Just. Kerner. Tübingen, 1820. 8.

Das Fettgift oder die Fettsäure, und ihre Wirkungen auf den thierischen organismus, ein Beitrag zur Untersuchung des in verdorbenen Würstengiftig wirkenden Stoffes, von Dr Just. Kerner. Stuttgardt und Tübingen, 1822. 8.

† Die neuesten Vergiftungen durch verdorbene Würste, beobachtet an 29 Menschen in und um Murrkardt im Königreich Wirtemberg, nebst dem Versuche einer physiologisch-pathologischen Darstellung der Einwirkung dieses Giftes auf den Menschen, von Dr Weiss, mit Vorrede und Anhang begleitet von Dr Kerner. Karlsruhe, 1824. 8.

ralysis of the branches of the sympathetic nerve, and also of the nerves of the head, which are not enumerated among the nerves of proper sense, presently follows. Debility of the heart and lungs, arising from loss of power in the muscles of respiration, succeeds, with paralysis of the bowels and of the abdominal muscles, alvine obstruction, sometimes diarrhœa and meteorismus. The action of the heart is slow, oppressed, and irregular. All the secretions and excretions are suppressed or diminished, excepting the urine, which may be increased. Sometimes, however, there is ischuria. The pupils are dilated, but of unequal magnitude. Vision is double, and the superior palpebra is paralyzed. The mucous membranes are much affected; and the inflammation becomes, as Kerner expresses it, asthenic, and are followed by the usual consequences,—thickening of the secretions and pseudo-membranous depositions. There is cough, similar to what is heard in *Angina membranacea*, with hoarseness or aphonia. The œsophagus becomes dry, loses all sensibility and muscular power; and hence dysphagia is formed. There is severe hunger, much severer thirst. The palms of the hands and soles of the feet become dry and horny; and the skin cold. The muscles of the inferior extremities are wearied but not paralyzed, though sometimes convulsed. The sensorium is not much affected. There is sometimes vertigo; watchfulness or somnolency; cephalæa with languor; and syncope.

The necroscopic appearances deduced from the examination of thirty-six bodies are generally as follow:

Cadavera very rigid. The blood-vessels of the dura mater, as well as those of the brain, were turgid with blood. Effusion of blood or bloody serum into the ventricles, or within the cranium. In two cases there was ramollissement of the brain. The thalami of the optic nerves, the retina, nervus sympathicus, nervus vagus, nervus recurrens, and ganglion Gasserii were greyish. The œsophagus was sometimes inflamed. The coats of the stomach much thickened and pustulated. The villous coat of the stomach and intestines often inflamed, and not unfrequently gangrenous. Meteorism of small intestines. Fæces hard and scybalous. The liver gorged with blood, and the gall-bladder preternaturally full of bile. The larynx and trachea inflamed, and filled with frothy mucus. The lungs abounding in blood, inflamed and hepatized. Carditis or laxity of the heart; polypi or thick blood in its cavities. The internal coat of the aorta yellow. The kidneys inflamed, and bladder also. The pupils permanently dilated.

Among the remedies employed by the German practitioners in these cases, emetics and laxatives are said to have answered best. When dysphagia was present, "the instrument of Professor Monro for emptying the stomach" was chiefly useful, although Dr Horn adds, that more frequently the lower part of the intestinal canal was affected with paralysis. In the inflammatory state of the fauces and pharynx, leeches were used advantageously. In the state of languor, Kerner prescribed the varied use of alkalis and acids, chiefly externally in baths, combined with laxative injections. Among remedies of this kind are particularly mentioned, hepar sulphuris, and vinegar. In the absence of dysphagia, the free use of sugared water with white of egg was recommended for drink, and crude eggs for food. If *paralysis pulmonum* supervene, Dr Horn advises artificial respiration, as a means only, however, of retarding death. One author states, that he had treated some cases successfully with oily remedies, to which were added, *natrum sulphuricum* and oily clysters.

The opinions of authors as to the nature of the poison have been very various. Dr Horn refers them to the following heads:—Poisonous seeds; hydrocyanic acid; pyroligneous acid; Welther's bitter; spermaceti, &c.; acids of different kinds of fat; putrefaction; previous disease in the animals of which the farcimina were made. Kerner concludes that it is a combination of acid of fat, with a virulent substance, of the nature of an alkaloid or Welther's bitter. Buchner, however, denies that the experiments of Kerner warrant the conclusion that the poison is acid. On the contrary, he says, they manifestly show that the poison has the properties of a base; and that the acid is nothing more than the acetic combined with a substance having the appearance of æthereal oil. Buchner killed many animals with the æthereal oil, which remained after the destructive distillation of fat; but the acid of fat, as it is called, never acted otherwise than as acetic acid. Buchner appears to have given the name of[*] "Pimelinum" to a fluid product, which he obtained by the destructive distillation of fat, endowed with the properties of a base. MM. Horn and Schrader could not succeed in obtaining it.

It may be proper here to remark, that Kerner obtained what he considers acid of fat from some farcimina which gave proofs of acidity. He distilled them with water, and thus obtained a fluid acid, which precipitated a solution of nitrate of silver in a white flocculent form, which soon changed to yellow. Some drops of the acid, much diluted, produced an astringent acid taste, with great dryness of the mouth and fauces, and a sense of constriction about the glottis.

Saturated by an alkali, and separated by sulphuric acid, it coagulated saliva like blood, leaving scarcely any serum. Muscle did not putrify in it. Kerner believes that he has found the same acid in blood passing into a state of putrefaction. He views it also as the same which is found in poisonous milk, cheese, shell-fish, &c. and purposes to call it *acidum cadaverum*. According to Dr Horn, Kerner considers it identical also with Bertholet's *acidum zoonicum*, and Chevreul's *acidum margaricum*.

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[*] From πιμελη, pinguedo, adeps, I suppose.—R. A.

Buchner experimented with the same acid on animals without any injurious result, prepared, however, by a different process. And Pfaff ascribes the above-named precipitate of the nitrate of silver to the presence of muriatic acid, from the culinary salt used in the preparation of the sausages.

Some farcimina, which were known to be poisonous, were sent to Buchner*. Distilled water from a sausage of this kind showed the presence of acetate of ammonia with a certain animal matter, six drachms of which were given to a little dog without producing any injurious effect. The remainder of the liquid having become putrid was given to a pigeon with the same result. The brown liquid remaining in the retort after the distillation of the aqueous infusion, inspissated to the consistence of extract, produced no injury in a dog. Hence Buchner concluded that the fixed parts soluble in water were not poisonous.

A spoiled sausage made of liver was cut very finely and repeatedly digested in highly rectified spirit of wine. The liquid was strained when cold, evaporated to the consistence of an extract, and furnished a mass which weighed ʒij. xxv. gr. of a most fœtid taste, pungent, and salt, and which produced most remarkable dryness of the fauces. Part of this mass, amounting to about ℈ij. was insoluble in water, and this Buchner held to be the *venenum farciminale*. Half a drachm of this mass killed a whelp of three months in thirteen days.

Buchner was induced by his experiments to infer that the poison of corrupted farcimina consists in a spontaneous degeneration of fat arising independently of the access of air. Dr Horn remarks, that this is not advancing beyond the opinion formerly expressed by Kerner: and, with regard to the experiment of Buchner just now alluded to, he says, it is not probable that the same poison concentrated would operate so slowly on a whelp, which in two slices of a sausage composed of liver would seriously derange the health of a person for many years. Buchner does not seem to have succeeded in insulating the poisonous principle.

With the view of illustrating this part of the subject, Dr

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* Buchner's Toxikologie, Zweite Auflage. Nürnberg, 1827.

Horn, in conjunction with his friend, M. Theodore Schrader, a skilful pharmacien of Berlin, engaged in some experimental researches on fat. The following is a translated recital of their labours.

Eight ounces of fresh hogs' lard were distilled in a suitable apparatus. The fat having gently boiled for some time, gas was disengaged, which communicated to the water through which it was transmitted an acrid odour, which affected the eyes powerfully. A clear oily fluid passed into the receiver. When the gas ceased to come over, and some carbon remained in the retort, the apparatus was cooled and separated.

The distilled oil, at a temperature from 8°—10° R. acquired in a short time the consistence of fat, weighing ℥viiss. ℈ss. About ʒj. of a watery fluid passed over at the same time, which had acid properties, and, like all the other products, an acrid odour. On repetition the experimenters found that the greater or smaller quantity of this watery fluid depended on the temperature at which the lard was distilled, so that the greater the flame the more fluid was the product, and the less passed over, because the more gas was disengaged.

The distilled fluid had a strong acid re-action with litmus paper, and had the above-mentioned acrid odour. The water through which the gas was transmitted had very acid properties, and presented the following relations to re-agents:—

Muriate of Baryta.	No change.
Acidulous acetate of lead.	White precipitate.
Subacetate of lead.	The same.
Nitrate of the protoxide of mercury.	Slightly opaque, gradually a black precipitate.
Nitrate of the peroxide of mercury.	A little turbid and white.
Nitrate of silver.	No change.

The acrid odour remained after the precipitation by the lead. Buchner, indeed, contends that in distillation there is a combination of the acid with a base, which, by distillation with caustic alkali, he had disengaged, and to which he thinks the poisonous nature and acrid odour are to be ascribed. This experiment we have repeated with all possible diligence and precaution; but nothing came over but the watery fluid, without any base but rather with slightly acid properties, and æthereal oil—both of which retained the acrid odour. And this experiment, however often repeated, never yielded any product indicating the presence of a base.

We now began to distil some of the oily product with water, in order that, neither muriatic nor sulphuric acid having been employed, we might obtain a pure acid of fat, which should accurately indicate its relations to the metallic salts. That the acrid pungent matter was not combined with the oily product may be concluded from the circumstance, that the water through which the gas had been passed, retained after the precipitation with lead the acrid odour which it possessed before, even after having been mixed with sulphuric acid or potass.

This distillation liberated a good deal of æthereal oil, which passed over with the water. The same pungent odour existed in this æthereal oil, which was easily soluble in cold spirit of wine, and indicated acid properties. The water which passed over had the same odour, but was much more pungent, and indicated a high degree of acidity.

The following were its relations:—

A glass rod moistened with ammonia held over it did not liberate any vapours, and the same was the case with acetic acid.

Baryt. acet.	No change.
Plumb. acet. acidul.	
Plumb. subacetas.	Slight muddiness.
Hydr. proto-nitr.	No change at first, presently a trifling precipitate.
Hydrarg. pernitras.	A little turbid and white, gradually changing into tawny.

Hence in water distilled with the æthereal oil, acid of fat, properly so called, is not found, seeing that its three most decisive indications are absent, viz. its capacity of precipitating the salts of lead, silver and quicksilver, white: on the contrary, there was a dark precipitate with the salts of quicksilver. Nor is the muddiness occasioned by the *plumb. acet. basic.* of greater moment, as it always occurs on dilution with pure water.

One pint of water was distilled with ʒii. of the product of fat.

The water which remained with the oil in the retort undistilled had now the peculiar empyreumatic odour, but it affected the eyes very slightly. It exhibited the following phenomena:

It reddened litmus paper.

Baryt. acet.	No change.
Plumb. acet. acid.	Immediately a white precipitate.
Plumb. subacetas.	
Hydr. proto-nitr.	
Hydrarg. pernitras.	
Argent. nitr.	Immediately a white precipitate, not, however, so copious as with the salts of lead and quicksilver.

That much of the acid was present in this water the reagents fully showed. We afterwards prepared the acid in the following manner proposed by Berzelius.

The product of the dry distillation of ʒiii. of lard was boiled in a retort with a sufficient quantity of water, (in order that at the same time the acrid pungent water might be obtained,) and some coarsely powdered marble for about an hour. The distilled water indicated acid properties, although there was plenty of marble, and acted in the same manner with reagents as though it had been distilled without marble. Æthereal oil also passed over. But, finding that the fluid in the retort still acted acidly, we continued the boiling in an open vessel, having added more marble. Nor did these acid properties disappear, although it was boiled with an excess of marble during many hours. The fluid when cold was strained and saturated with muriatic acid. The precipitate thus obtained being well washed weighed fifteen grains.

The acid thus obtained was perfectly white, pulverulent, but still was a little affected with the empyreumatic odour. Moderately heated in a platina spoon it became fluid, began to burn with a bright flame, and left a little carbon. Heated in a glass tube, which was hermetically sealed below, the mass assumed the form of vapour, white and acrid, which adhered to the cooler part of the tube in the form of a film. The liquefied mass left to cool assumed the form of acicular crystals. Pure benzoic acid yielded a vapour much more acrid, which in the cooler part of the tube crystallized in long needles; the cooled mass formed a fasciculated, villous kind of crystallization. Cold water with difficulty, warm water easily, dissolved this acid, but, like the benzoic, when cooled, it resumed the crystalline form *.

We could never succeed in investigating the properties of the pungent and very volatile matter of the oily product, for it was in vain that we tried to collect it. It appeared to have more affinity for water than for the æthereal oil; and whether it be of the nature of a base, could not be demonstrated, because it could never be separated from the volatile acid, which neither formed precipitates with the salts of lead or silver. In order that we might ascertain whether this matter could be obtained in any other way than by solution in water, some of the oily product of lard was distilled in a retort, the receiver of which was surrounded by a refrigerant mixture. Then the retort being gently heated, the receiver was continually surrounded with fresh supplies of the refrigerating mix-

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* It is an extraordinary omission that not a single proof is adduced to satisfy the reader that this white pulverulent substance is really an acid.—(R. A.)

Sebacic acid, first demonstrated by Thenard and confirmed by Rose, and even now called by that name by the French chemists, according to the more recent opinion of Berzelius, is nothing else than benzoic acid rendered impure by a certain organic matter, and which differs from benzoic acid in this, merely that it precipitates acetate of lead and nitrate of silver white, which the pure benzoic acid does not.

ture, so that a temperature about 3° R. was preserved. Nothing but a pellucid æthereal oil passed into the cucurbit, which contained dissolved a great part of the volatile matter. Now, although the odour, as described, of the poisonous farcimina has no similarity to the pungent matter in all these products, nevertheless the following experiments were undertaken to ascertain what similarity might be found in their effects.

A. Experiments instituted with the products of the dry distillation of fat. A very weak Pomeranian whelp swallowed gr. xxxiv. of a mass, composed of equal parts of the above product and *pulv. Altheæ.* Cough, but no other indication of disease, followed. After the lapse of four days, ʒiss. of the same mass was administered. A severer cough occurred, but he immediately returned to his food, and went about as usual; nor did accurate examination of him for several days discover anything contrary to a healthy state in him.

Two drachms were given to an old but healthy horse. No change was observed in any of his functions during five days, in which he was closely watched.

B. Experiments instituted with the product of the oily distillation with water. ℥iv. of the oily product being distilled with water (to which that water was added, through which the gas was transmitted in forming the oily product) one pint of water was withdrawn.

1. Flies which ran about on frogs killed by this poison, died not on the same day, but after very few days.

2. ʒss. of this substance was dropped into a brisk adult *rana temporaria*, some of which escaped. Strong spasms appeared, which presently ceased; respiration became difficult. The power of motion remained. He took no notice of flies. He remained during four hours with the anterior extremities stretched out, and pouring out from the mouth much coagulated fluid, and then died.

Nothing particular was observed on examining the viscera, nor could the poison be recognized by its odour. Kidneys very red; the upper half of the body filled with blood; vessels generally empty, excepting those of the intestines. Contents of stomach and intestines fluid; body rigid.

3. Two adult frogs, but rather languid, each of which had swallowed ℈i. of the poison, began to be affected with severe convulsions, which, however, ceased, so that after two hours there was nothing particularly observable, excepting disturbed breathing, the capability of moving not being particularly affected. They disregarded the flies thrown to them, but leaped about as before. One died after seven hours, no particular symptom having occurred; the other in two hours, paralysis

of the inferior extremities having preceded. The saliva was coagulated in both.

The body was rigid; the odour of poison was not recognized. In the intestinal tract, and especially in the stomach, air and a frothy fluid were found, but without any odour of the poison.

4. Two small, lively, but not adult frogs, were sprinkled with ℈ss. of the substance, and died after two hours with severe convulsions.

5. ʒss. of the poison was given to an old pigeon, greater part of which escaped from the beak, the remainder being swallowed. Immediately the breathing became difficult, loud, shrill and slow, but in an hour was again free. The pigeon with its feathers up did not remove, nor did it notice the water offered to it, but frequently voided feces of a natural appearance. The pupil remained mobile. After three hours he vomited some peas, became quiet and drank, remaining motionless in the same place. After twenty-four hours ʒss. more was given, and the same symptoms returned. Immediately after receiving the poison he rejected a good deal, and shortly afterwards a frothy and coagulated matter appeared in the beak. The pigeon ate some peas and drank eagerly, but with difficulty. On the following day some more drops of the same fluid were administered, after which the respiration became more difficult, and so continued till the evening. Lastly, a portion, though smaller, produced a severer effect. He neither ate nor drank; breathed laboriously, and drops of a yellow fluid hanging from the beak. Twelve hours after death the body being examined, the brain and nerves were found unaffected. The mouth and fauces had a yellow moist covering. Neither in the trachea nor lungs was any thing remarkable to be observed, excepting that here and there they seemed inflamed. The internal tunic of the œsophagus was so dissolved that it might be wiped off, as it might be also in the upper part of the intestinal tract. For the third of an inch before the commencement of the stomach a compact yellow tunic appeared, which at the commencement of the stomach had a dark green colour, and passed into the duodenum in a sacculated form. The duodenum being dissected, presented fibres of the same colour and some small stones.

6. A healthy adult pigeon, to which ʒss. of the poison was given, rejected the half. After this the eyes became strongly inflamed; the respiration exceedingly difficult, so as to be heard at the distance of some steps. A yellow fluid and thick mucus appeared on the beak. After twelve hours he was much weaker. He had not taken food; breathed more freely. On the following day he remained stationary in the same place, the respira-

tion continuing difficult. Thirty-six hours after the first poisoning an equal quantity was given, the breathing became vehemently stridulous. After some hours he died tranquilly.

On examination twelve hours after death, the body was found rigid, the viscera natural. In the rectum there were much fæces green and solid. The inner tunic of the œsophagus and duodenum had a greenish yellow appearance. The stomach the same as in the former birds. The liver turgid with blood, as were also the kidneys and lungs. The heart, excepting the left ventricle, full of black coagulated blood, and also the large veins. Brain natural. On the first poisoning of this bird, the eye, from the accidental introduction perhaps of a drop of the poison, was closed, nor was it opened again during life.

7. ʒvi. were given to a healthy white rabbit, which had fasted eighteen hours; great part was rejected. Immediately the respiration became very difficult, and so continued for three hours, the head being drawn backwards. Neither urine nor feces were voided. He died tranquilly four hours after the poisoning.

Twenty-four hours after death the body was rigid, the skin and muscles abounding in blood. The surface of the brain and its meninges were very red; the internal part and the spinal marrow were natural. The lungs, of a whitish red, were in some parts near the heart inflamed. The cavities of the heart were generally filled with black coagulated blood, which in the vena cava were like polypi. The sympathetic nerve was natural. Stomach inflated with air, contained the remains of leaves, which smelled strongly of the poison. The internal tunic dissolved, in some places inflamed, as was also the superior part of the intestinal canal.

8 and 9. A fat old dog of the kind called Teckel, took ʒiii. of the poison. He became sick, but did not vomit. Shortly after he seemed well. In two days he died. The author did not examine the body of this dog, nor did he see him during the two days alluded to.

A Pomeranian dog took nearly ʒvi. of the poison, the whole of which he did not retain. He soon lay down, respiration becoming rather difficult, and the dog languid. Vomiting did not occur. In two days he died. He was not more particularly observed, nor was any *post mortem* examination instituted.

Both the latter experiments, although deficient in accurate observation, and therefore not affording grounds for the establishment of a law, plainly manifest that strong healthy dogs may be killed by this fluid. For it is certain that these dogs both before and after the poisoning took no other injurious substance.

C. Experiment instituted with the æthereal oil (ʒiii. gr. xiii.) liberated in the distillation.

The whole quantity was given at once to a middle sized Pomeranian dog, but a great part was rejected. He immediately fell down and breathed so as to be scarcely heard, and very slowly. He was slightly convulsed. In a short time he became better. The head was violently agitated, but he did not vomit. Dr Horn could not accurately watch this dog, and adds only, that he was alive in eight hours after the poisoning, but was dead on the following morning.

D. Experiments made with the pure acid of fat. *

1. ʒss. of this acid was given to a strong old horse made into a bolus with *pulv. altheæ*. For six days he remained well, and being then killed his viscera were all sound.

2. ℈ss. was given to an old spaniel without any injurious result.

The author did not think it worth while to prosecute this subject farther. He draws the following conclusion: that the pure acid of fat has no poisonous action, hence it cannot be the "*venenum farciminale*."

Dr Horn terminates his essay by saying, that he cannot concur in any of the opinions put forth as to the nature of this poison; and that it appears to him to be some matter developed by spontaneous putrefaction, and that this spontaneous putrefaction arises perhaps in improperly made farcimina.

It may be safely said, however, that putrefaction is not the proper term to apply here, for it is distinctly stated in the essay, that farcimina, known to be poisonous, were not to be distinguished either by their odour or taste. Some kind of degeneration may be correctly assumed; and it is highly probable that a principle of a peculiar nature, and poisonous, is thereby developed. What that principle is remains to be demonstrated. Buchner's name of Pimelinum involves an hypothesis at present inadequately supported by experiment.

Coventry, July 1, 1829.

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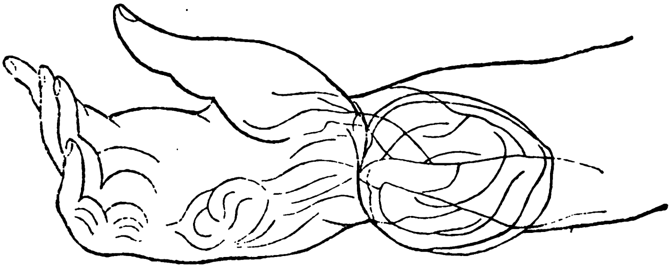
* How this acid was obtained is not stated. It appears to have been a fluid, and not the white pulverulent matter previously alluded to.

ART. IV.—*Case in which Hydatids were contained in the Synovial Sheaths of the Flexor tendons of the Hand.* By JAMES RANKINE, M. D. Licentiate of the R. C. S. Ed.



MRS ——— aged 33 years, about five feet six inches high, of a spare but active make, about four years ago was seized, without any assignable cause, with a severe pain situate in the top of the right shoulder, shooting down into the forearm, from which it passed into the hand and wrist, affecting the fingers with much numbness, the two last of which were very painful and drawn down. For the first six months or more the pain of the whole arm, but particularly of the hand and wrist, deprived her of rest and comfort, especially when in bed, which she attributed to the warmth of the bedclothes; and from which circumstance she was persuaded by her neighbours that the disease was rheumatism, and had nothing done for it. One afternoon, all of a sudden, the back of the hand swelled to a considerable size and broke out into a large crop of red pimples, from the size of a pin point to the size of a pin head, which eruption much relieved the pain of the wrist and fingers. She was advised to apply a poultice over the swelling, which completely reduced it. The whole disease now settled *simultaneously* in the front of the wrist and *palm* of the hand, and kept gradually increasing until it attained its present size. There was very little pain for the last three years, until about six months ago, when she began to suffer considerably. During the whole progress of the complaint her general health has continued unimpaired, and she has all along remained at her employment, which is painting in the pot-works. She was in the family way when the complaint began, and has had three children during its continuance.

On the 7th of July, Mrs ——— presented herself to me with the above disease (an outline drawing of which I have prefixed.) I found upon examination a large tumour about the size of a middling orange, flattened, situate under the fascia of the forearm immediately above the part of the wrist, principally on the ulnar side, and close to the annular ligament, by which it seemed to be confined and bound down. In the palm of the hand there appeared another tumour about the size of a large pigeon's egg, likewise situate beneath the fascia. This tumour had an irregular knotty feel, and when pressed, its contents passed easily into the larger tumour, which might be acted upon in like manner. The tumours were not at all discoloured, and the large one had a peculiar doughy, irregular, feel, with varicose veins all over its surface and upon the parts adjacent. I had some doubt as to the nature of the tumours, and the propriety of opening them, fearing that their contents might be effused blood, particularly as they had been attended with considerable throbbing pain, and as they had not the evident fluctuation attending the presence of contained matter. I resolved to proceed cautiously.

Pressing upon the larger tumour, therefore, and causing the smaller one to become hard and tense, I pushed a thick tailor's sewing needle into it, which upon being withdrawn was followed by a small discharge of glutinous amber-coloured liquor. Encouraged by this result, I opened the tumour with a small lancet, and, to my utter astonishment, upon being pressed, out bounced, like peas from a pop-gun, more than a coffee cupful of egg-shaped opaque bodies, which upon examination proved to be hydatids from the size of a small pea to that of a middle-sized gooseberry. The relief felt was immediate, and the patient could now extend her finger for the first time for a long period. There was no discharge except these hydatids, and the blood arising from the puncture. I now bandaged down the loose integuments of the large tumour with a roller, ordered a poultice to the palm, and left my patient.

Examination of the Hydatids.—I selected one of the largest, and cut it open by a longitudinal incision, which showed a thick opaque capsule or bag in oinion-like layers, containing a melicerous or calf's-foot jelly-like matter, which being subjected to a microscope of the power of 1000, appeared vesicular and silvery or shining; but when put into the focus of a compound microscope of the power of 6000, showed a complete vascular like net-work. There were four mother hydatids or cysts, which had apparently been burst by being pressed through so small an opening. They consisted of a substance not unlike soft macerated cartilage, the largest having one cavity filled with young hydatids from the size of a pin head to that of a large pea, which were adhering to its sides; the others were smaller, and had more than one cavity, likewise filled with young hydatids. Except when fully grown the individual hydatid showed no internal cavity, but was within of a uniform, pulpy, gelatinous, nature. Whether these hydatids are animalculæ, as some have pretended, and that they belong to a genus of which they assure us there are different species, I cannot presume to decide; but in this instance they certainly appear to me to have been the production of disease; and it must be confessed it is rather a stretching of the known laws of animal life, to suppose that a series of the same animals can be inclosed in one another like pill-boxes, as in some instances have been asserted.

Remarks.—It being impossible to know precisely during the life of the patient where these hydatids were situated, it may be conjectured that they occupied one or more of the bursæ about the joint on the sheath of the flexor tendons of the fingers, and that they were the result of the slow chronic inflammation to which these tissues are sometimes liable. In the first instance, it is not improbable there was a general acute inflammatory affection of the muscular fasciæ of the whole arm, which being left to chance, partially relieved itself, and then supervened this chronic inflammation of the bursa or bursæ, or sheath of the flexor tendons, which in the course of time, without pain or uneasiness, formed and maturated this collection of hydatids; for it was not until within the last six months my patient complained of any pain, (omitting the first year,) except occasionally a little aching after a day's washing. To suppose that they were contained within a capsule, the result of inflammation in the cellular substance, I apprehend, would not sufficiently account for the peculiarity of the symptoms,—the acute and peculiar pain subsisting for about the space of twelve months,—the two years and a half absence of pain,—and the gradual enlargement of the tumours, and the recurrence of considerable pain when the tumours attained so great a size as to press upon and extend the aponeurotic expansion of the wrist and palm. Indeed the cell in which the hydatids found their domicile can now be distinctly felt situated behind, and seemingly around, the flexor tendons of the fingers above the wrist, and to have followed them through below the annular ligament to protrude in the palm of the hand, when the tumour was covered by the palmar fascia, which appeared, when the fingers were forcibly extended, white and glistening. In enlarging the opening the following day, I wounded one of the vessels of the palmar arch, and could easily distinguish by my probe that I had got amongst the flexor tendons, from whence the hydatids appeared to have been expelled.

Hanley, September 12, 1829.

Art. V.—*Observations on the Epidemic Eruptive Rheumatic Fever of the West Indies, as it occurred in St Bartholomew.* By W. H. Cock, M. D.

Dear Sir,

In compliance with a promise which I some time ago made you, I shall now trouble you with my observations on the disease which so universally prevailed in the West Indies in the year 1827.

These observations will be confined to the description of it as it occurred in my own practice; to its origin, whether contagious or not; to the treatment which I pursued; and I shall conclude with a few remarks on its nature. This disease, which was vulgarly known by the name of the Dandy, made its appearance in the Island of St Bartholomew in the month of November 1827, and prevailed throughout the months of December and January.

The attack was sudden. It commenced by heavy sensations in the eyes, frequent yawning, and redness of the countenance, which were soon followed by shivering, pains in different parts of the body, as the back, the joints of the fingers, toes, knees, &c. The severest pain was in general that experienced in the head. It was in most cases confined to the fore part; but not unfrequently affected the whole. The eyes were ferrety, and exhibited much the appearance of intoxication. Such was the excruciating nature of these pains, that few had fortitude sufficient to support them without complaint. In my own case the pain of the head was very acute and distressing; and I suffered much from pain in the left humerus, which at the time I compared to the sensation of a pointed instrument boring the bone. The burning torture of a mustard cataplasm, which relieved this most dreadful of all pains which I ever experienced, was so grateful in comparison, that during its operation I fell into a gentle sleep.

The pain in the head, although constant for the first twenty-four hours, was not, however, always equally severe; but would frequently subside a little to return in paroxysms of redoubled violence. The lobes of the ears were likewise greatly affected and pained. The toes and fingers frequently suffered much; and when the treatment was not active, remained swelled for several months. The pains of the back were excruciating, and the sensation was as if the vertebræ were torn with violence from each other.

The stomach was in general but slightly affected. The skin in all cases was dry and parched, the heat above the natural standard, and accompanied with a burning sensation. The tongue was loaded and covered with a white crust, and all inclination for food entirely lost. The pulse was variously affected in different cases. The urine was high-coloured and the bowels confined. These symptoms, which may be considered as those of the first stage, continued violent from twenty-four to thirty-six hours, when they gradually subsided, the pains in the joints being the last to leave.

About the third or fourth day, in many cases, an erythematous eruption appeared on the hands and feet, accompanied with swelling of those parts, and extending over the rest of the body, continued for about thirty-six hours, when it faded, and the cuticle peeled off as in scarlet fever, leaving a considerable degree of soreness. The soles of the feet were sometimes rendered so sore, that walking for many days after restoration to health was attended with pain. A slight febricula was present during the eruption. In many cases the pains again returned after the eruption, and, fixing themselves in one or two joints, remained for several weeks and sometimes months. These pains were not accompanied by evident fever, although I think I noticed the perspiration to be but partial, and the skin in the mornings and evenings drier than what it is in general in this climate. When, however, the practice was sufficiently energetic, the eruption was not only much modified, but these after-pains constituting the rheumatic stage, were generally prevented. Many, however, of the cases in the town ran through all these stages. These were in general such as occurred when medical assistance was not to be had, the physicians being all at the same time attacked by the prevailing malady. When this last stage was established, it was very difficult to remove, and remained in some instances for months attended with swelling and stiffness of the joints. In one case a partial ankylosis took place in the fingers of the hand. These pains were the most severe in the mornings and evenings. Although the fever was considerable, yet during the first or active stage of the disease the patient frequently complained of cold, and demanded an addition to his coverings. In some cases the glands in the groin were very painful and swelled.

The attack, as I observed, generally commenced with heavy appearances of the eyes, and a flushed countenance, which was frequently perceived by your friends, who would thence predict your attack before you felt the least symptom of pain. This, however, was not always the case; for pain in the fingers and toes would sometimes precede every other symptom. In one instance the whole force of the disease was confined to the extremities. The head was not affected, and there was no pain in the back. The pains shifted from one part of the patient's extremities to the other; sometimes attacking him in the knees, sometimes in the toes; sometimes in one toe, sometimes in another; and each invasion of pain was accompanied with twitching of the muscles of the part attacked. The shifting of the pains from one part to the other was attended with an interval of perfect ease. A patient might be conversing calmly with you, when he would suddenly cry out from the severity of the returning pain. This was a solitary case; but in the course of three days, by the use of purgatives, warm bath, sudorifics and frictions, he was restored to perfect health, and never after experienced any pain.

In children the attack was generally mild, and the third or rheumatic stage was wanting. I do not recollect any fatal case which occurred in children. One child, however, not quite a year old, was very severely attacked, and remained in a comatose state for several hours. He recovered by the use of emetics and calomel. Twenty grains of ipecacuan given in divided doses had no effect until five or six grains of calomel had been exhibited. Another child of three or four months was attacked with spasms and twitches of the tendons, and screamed much when touched; but by means of warm bathing, emetics, and laxatives, he soon recovered. The disease proved fatal in some few elderly negros, and in one or two broken down constitutions. I did not observe that those who had formerly suffered from rheumatism were more liable to the after-pains than those who had never experienced that disease.

I now come to consider the origin of the disease, whether derived from contagion or from an epidemic condition of the atmosphere. I was first disposed, from the rapidity of its progress, and the universality of its attack, to attribute it to the latter cause; but more close investigation of its progress, and the consideration of the circumstances which attended its appearance in the neighbouring islands, have led me to regard its origin to be contagion. I afterwards ascertained that it was introduced into this island by a sloop from St Thomas, which arrived here with some passengers sick of this disease; and from that moment it began to prevail. It was at first confined to that portion of the town generally inhabited by the sailors; it afterwards gradually extended to other parts, until every house in town became infected with it. The attack of one inmate in a family was soon followed by that of all the others. In my own family, my second daughter, a child of four years of age, was the first who was attacked, and as her fever was intense, I remained with her the whole night. When holding her in my arms, I felt on one occasion a singular sensation, as if a pungent heat had penetrated my body, and was diffusing itself throughout, which occasioned me to observe that I had taken the disease. On the following morning, I was severely attacked, and my wife, who remained in constant attendance on me during that day, was the next day seized in a similar manner. Not one individual in my family escaped. From the town it spread to the country, first commencing with those whose occupations rendered daily communication with the people of the town necessary; and then gradually extending itself, according as the friendly relations between different families brought them in contact with the sick.

As the disease was not considered dangerous, or in the beginning supposed to be contagious, no precautions were observed; and this unrestrained intercourse was the principal cause of its rapid diffusion. Many of the neighbouring islands were indebted to us for their acquaintance with this singular malady. Among others, the Island of Anguilla, to the north of this, belonging to the English, and which is principally supplied by our merchants with most of the necessaries of life. From the frequency, therefore, of our mutual intercourse, it was natural to infer, that if the disease were contagious, it would not be long before it made its appearance among the inhabitants of that island. Such was actually the case; and the first individuals who there exhibited specimens of this disease were among those who had recently left St Bartholomew. Another proof here presents itself in confirmation of my opinion. In Anguilla, the inhabitants, following a rural life, are removed to considerable distances from each other. The circumstance, therefore, which principally contributed to its rapid progress in the other islands, viz. frequent communication with the sick, was there wanting; hence the disease by no means embraced the whole of that island, but was principally confined to that portion which preserved the most frequent communication with this island. Had it originated in an epidemic state of the air, would it have been so partial in its selections?

I may adduce also the instance of the Island of St Christopher, which lies in a northerly direction from St Bartholomew. In that island the disease was entirely unknown, until the return of the sloop Maxwell, from our port, when it made its appearance in the persons of three of the passengers, who had been down in her to St Bartholomew on business, and from them it soon spread with rapidity through the island.

With these observations, I now dismiss the question of its contagious nature, and shall proceed, in a concise manner, to state the treatment which I found the most successful, and which was regulated by no particular theoretical views, but directed entirely to combat the symptoms, which appeared to be the most distressing or dangerous. To observe some order, I shall first state those means which I found of most advantage in the first stage, and proceed to point out those I employed in the eruptive, and lastly, in the rheumatic stages.

To commence with the first or active stage of the complaint, the symptoms which were the most distressing were the acute pain of the head, the pain in the back, the pain in the joints, and the burning and painful feeling of the skin,—symptoms indicating high phlogistic action. The practice was therefore depletory; and as the most powerful means which we possess of reducing action is venesection, this claims the priority of consideration. Its advantages were very decided, and seldom failed when properly performed, to remove the pain of the head, and considerably to alleviate the other sufferings of the patient. The pains in some hours after returned, but in a less violent degree. I never, however, had recourse to it, but when I found the pulse high, or contracted and oppressed, the eyes red or heavy, and the flushing of the face considerable. After bleeding I immediately administered a strong and active cathartic, which was followed twelve or fifteen hours afterwards by a warm bath, when this could be obtained, and by some sudorific beverage. Cream of tartar, to which a small quantity of tartar emetic had been added, or some warm herb-tea mixed with a small proportion of antimonial wine, and drank warm, generally promoted gentle perspiration, which considerably removed the pains. On the following day the cathartic was repeated, and if the pains returned, active purgatives during the day and sudorific drinks were continued until every symptom had subsided. I have thus, in some cases in three days, restored my patients to health; and on the fourth day from the attack, they were attending their ordinary business, and free from all pain.

Many, however, could not be induced to take sufficient purgative medicines, and these passed through all the stages of this disease. Bleeding, in short, when performed under the circumstances above-mentioned, and followed by purgatives and sudorifics, frequently prevented entirely the rheumatic stage, or so modified the pains with which it was accompanied, that they consisted in a feeling rather of a slight degree of stiffness in the morning and evening, than in any real pain, and easily yielded to a smart purgative, which was followed by some gentle sudorific. When the determination to the head was not so urgent as to require bleeding, an emetic of ipecacuanha with tartar emetic was administered; and its operation was always followed by great relief to all the symptoms. I sometimes premised a purgative, and then gave the emetic, and I think with more advantage than when I gave the emetic the first. The relief derived from emetics was very considerable, and, although not so speedy as that derived from bleeding, yet equally permanent; and when supported by the active and persevering use of purgatives, seldom failed in preventing the horrors and distress of the third stage. The emetic I sometimes gave twice during the first stage, but in general only once, and entrusted the rest of the complaint to purgatives and sudorifics, which seldom defeated my expectations.

The purgatives which were employed were of the most active description, and were repeated every twenty-four hours and sometimes oftener. To an adult male I never gave less than thirty grains of jalap with fifteen grains of calomel, or fifteen grains of calomel with fifteen grains of jalap and three grains of scammony. One of these purgatives was repeated on the following day, or some other substituted, as a combination of jalap, rhubarb, and nitre, or of senna, rhubarb, and salts, and the like. Every night I gave some sudorific, such as antimonial wine in some diluent drink, or Dover's powder, which was the only form in which I ever used opium. Warm baths were highly advantageous, and assisted much the other means. I persevered in this course until every pain was removed, and in five or six days, where this practice was pursued, had the satisfaction of seeing my patients restored to health with trifling, if any, symptoms of the third stage. The eruption, which was in general very slight when the above practice was followed, required nothing but perseverance in the same means. The pains in the joints, which in the first stage were often very severe, were best removed by frictions of anodyne liniment, the application of mustard cataplasms, and of blisters. The burning of the skin yielded to warm bathing, ablutions with warm water and vinegar, or with bay rum.

Such is the treatment which I pursued in the first and second stages of the disease; and if it could have been followed in every case, the symptoms of the third stage, which were the most distressing, from their long and obstinate continuance, would scarcely have deserved notice. From the obstinacy, however, of some who would not submit to this active treatment, and from the impossibility of others obtaining medical advice from the cause which I have already mentioned, many cases of the third stage after my own recovery came under my care. These were treated by purgatives, sudorifics, and alterative doses of calomel and James's powder, with the topical aid of stimulating frictions and blisters. When the pain remained fixed in any joint, great relief was experienced from enveloping the part in a strengthening plaster. Flannel in many cases was found of advantage. Powders consisting of powder of guaiacum, sulphur, and cream of tartar, were administered with some benefit. From cold bathing I never saw any relief, but, on the contrary, an increase of all the symptoms. Under the use of port wine in the stage of convalescence, the taste, appetite, and strength soon returned.

I am afraid that I am drawing considerably on your patience; but I must beg of you, before I conclude, to indulge me in a few observations on the nature of the disease. Is it then a new disease, or is it one with which the medical world is familiar, but so disguised by accidental circumstances as to appear under a new garb, which would, unless strictly investigated, conceal its true nature? That the combination of symptoms was such as has seldom been witnessed I will not dispute; but I think that the symptoms which characterize scarlet fever were so well marked, as to entitle it to be ranked as a variety of that disease. My opinion therefore is, that it was a variety of scarlet fever associated with rheumatic symptoms; and this opinion I found on the following reasons:—1*st*, The time of its appearance, viz. beginning of winter, corresponded with that of the scarlet fever. 2*d*, The period of the appearance of the eruption was, as in scarlet fever, about the third day from the attack. It disappeared three or four days after, accompanied with a separation of the cuticle. 3*d*, The hands and face swelled as in that disease. 4*th*, The treatment found to be most successful was similar to that which has succeeded so well in scarlet fever, attended with phlogistic symptoms, viz. venesection and purgatives. 5*th*, It was contagious. 6*th*, In the year 1828, the year after, in the months of November and December, many cases of *scarlatina simplex* and some few cases of the *scarlatina anginosa*, appeared in this island. There were likewise cases of pure rheumatism and sore throat. Many of the cases of the *scarlatina anginosa* were accompanied with the same symptoms as attended the disease in question. And I presume no one certainly will maintain, that, when sore throat is attended with violent fever, swelled hands and face, with scarlet eruption appearing about the third day and desquamating in the usual manner, the disease is not scarlet fever.

The symptoms of what was called the Dandy were as well marked in several of these cases as they were in any that occurred the year before, and there could be no doubt of their being genuine cases of scarlet fever, as the symptoms, with the exception of the rheumatic, were precisely similar to those which we should expect in that disease. An example will illustrate this. On the 28th December 1828, I was sent for to visit Miss B. M. I found her suffering from violent pain of head, pain in the back and legs, and excruciating pain in several of the joints. The right arm was so stiff and painful, that it could not be raised to the head. Skin very hot and sore; tongue slightly swelled and foul; throat much inflamed, and tonsils ulcerated; deglutition difficult; fever high, and pulse full. About the third day a scarlet eruption made its appearance, and in a few days after the cuticle separated in small scales, leaving the skin tender. The treatment was bleeding, purgatives, and emetics. Here we have certainly the symptoms of scarlet fever; but have we not likewise those of the dandy? There were several other cases in which the same combination of symptoms existed, but it would be exceeding the limits of a letter to enumerate them. In these cases the symptoms of scarlet fever were certainly united with those of rheumatism, and did not differ in any respect from the disease of the year before, except that they were attended with sore throat. Here, then, we have the *scarlatina anginosa* combined with rheumatic symptoms; and why may not the *scarlatina simplex* be found in the same company? The sore throat I do not consider as necessary to the existence of genuine scarlet fever, but as a mere accidental association; for in the year 1828 the generality of the cases of the scarlatina were unattended with sore throat, and there existed many cases of sore throat unaccompanied by any scarlet eruption.

From these data I think that I am justified in concluding, that the disease which existed in 1827 was nothing but the *scarlatina simplex* associated with rheumatic symptoms. This combination resulted no doubt from the changeable state of the atmosphere, and from the sudden transitions from heat to cold, attended with frequent showers, which characterized the months of November and December of the year 1827. If required to fix its place in a nosological arrangement, I should certainly rank it as an accidental variety of scarlatina, and if it would not offend the classical ears of some of my brethren, who seem anxious to proscribe every term, however long it has been sanctioned by use, which cannot claim a classical origin, I would call it *scarlatina rheumatica*. Perhaps Dr Good might prefer *Exanthesis rosalia arthrodynia*. With these observations I conclude, hoping that you will pardon the loose manner in which they are communicated, as my time is but little at my disposal.

ART. VI.—*A few Remarks on the Dandy which prevailed in the West Indies towards the close of* 1827 *and beginning of* 1828. By JOHN FURLONGE, M. D. Member of the Royal College of Surgeons, and of the Royal Medical and Medico-Chirurgical Societies of Edinburgh.

THIS part of the world was in the latter end of 1827 and beginning of 1828 visited by an epidemic fever of a nature hitherto totally unknown in these latitudes. Perhaps, therefore, a few remarks on its nature and probable cause may not be unacceptable to the readers of this Journal, more especially as a diversity of opinion exists upon these points.

I shall, as concisely as the subject will admit, give an exposition of my own sentiments, formed from a careful review of the rise and progress of the epidemic. I should not have had the vanity to publish my views, had I not within the last few days seen the brief notice of my friend, Mr Nicholson, and the more elaborate essay of my friend, Dr Stedman of St Croix, in two late numbers of this Journal. To the general accuracy of the description of the former gentleman, I have pleasure in subscribing, but most assuredly differ from both as to the idea of its contagious nature.

It is a remarkable fact, that for some considerable time previously to the eruption of the fever in Antigua, during the day the weather was in the extremest degree hot and sultry, and the evenings, as soon as the sun began to incline to the west, nights and mornings were most uncomfortably damp and chilly. The air, indeed, seemed saturated with moisture, and to such an extent, that, if one awoke during the night, he felt disagreeably chilly, with the extremities cold, and to the touch seemed as if the circulation were almost arrested, and the blood repelled from the surface by the damp cold. It may seem strange to assert that this took place under the intense heat of a tropical sun; but it is no less true that many in the best health found it necessary to have recourse to blankets, to render their beds comfortable.

Any one conversant with the laws of the animal economy need not be told that the intense solar radiation acting on the body during the day invites the blood to the surface, and by its genial warmth restores the balance of the circulation, till the shades of the chilly raw evening coming on, struck with almost aguish torpidity the cutaneous and distal circulation. The blood quitting the surface became again accumulated in the cavities of the chest and abdomen, *—next day to undergo the same revolution. This alternation continuing for some time, it does not appear astonishing that the result should be a form of rheumatic fever, accompanied with great gastric irritation, such as we have seen here, and most of us have felt. The efflorescence in this fever I considered as altogether symptoma-

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* See Dr Johnson's invaluable work on tropical diseases,—a work without which no medical man should come to the West Indies, and which contains more enlightened views on the subjects of which it treats than any work I have ever read.

tic of the gastric disturbance, and I observed that its intensity and extent were proportional to that disturbance,—that those who were known dyspeptics had the eruption more extensively than others,—that literary or sedentary characters, and those whose occupations were such as likely to derange the chylopoietic organs, suffered most from nausea, vomiting, and other symptoms of gastric disorder, and in such the eruption was always most extensive and marked. The best idea I can give of the eruption is to liken it to measles, only it was elevated with *papulæ* and wheals,—a sort of hybrid or intermediate product between nettle-rash and rubeola.

We know that animal and vegetable poisons of some kind, by inducing gastric derangement, produce efflorescences on the skin; and I see every reason to believe that vicissitudes of weather, producing unusual and unnatural determination of blood to the stomach, and thus deranging its functions, are quite sufficient to account for the phenomena. But even in Europe rheumatic fever is sometimes accompanied with an exanthema. An enthusiastic disciple of Broussais would no doubt see in this fever a "veritable gastro-enterite." Few persons in the populous town of St John escaped the epidemic,—an event not surprising,—as it is without exception one of the dampest, and perhaps the coldest towns at times in the West Indies,* being literally built on a general spring, so much so, that after a little rain, water is seen in all parts of the town bubbling from holes and crevices, like so many London fire-plugs; and in any part of the town at all seasons water may be found by digging a few inches. I must also mention that Chisholm is wrong in stating there are no swamps near St John's. He never could have seen the neighbourhood. We are surrounded N. S. and E. by swamps, rendering the air of course more hygrometric.

Having thus endeavoured, though feebly I confess, to give the fever an atmospheric or meteorological origin, it would be wasting the time of my readers to say any thing against its contagious nature. In my humble opinion it was no more communicable than a common phlegmon. All being exposed to one common cause, where is the wonder of its affecting all, so very few escaping? and its wide and almost (with us) simultaneous diffusion, are in my mind the strongest proofs of its epidemic or atmospheric nativity. I attended a family of nine whites, all of whom were attacked, with the exception of one young lady, who acted with much solicitude, as nurse, to father, mother, brother, and sisters, and yet she escaped; such an occurrence was not likely to happen, had the dandy been communicable. The epidemic spared neither age, sex, nor colour. I saw it in children eight weeks old. In families of ten or twelve persons, including servants, perhaps half or eight would lie down at once —a fact more characteristic of an atmospheric origin than the other opinion.

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* Dr Chisholm says, in his Manual, that St John "is distinguished by a peculiarity in its atmosphere. It is a degree of cold, not observed in any of the other islands." He states the thermometer being at night as long at 62° and above 100° in the day. He calls it a "singular coldness." For truth of his remarks I can vouch.

The dandy travelled in some degree mysteriously, over this Archipelago, from Barbadoes to Jamaica. It raged in America and in the East Indies. No disease, indeed, with the exception of influenza, ever had so wide a diffusion. From St Thomas it proceeded upwards to Barbadoes. But this is easily accounted for, from the fact of the wind being always N. and N. E. for a very considerable period prior to the eruption of the fever. By this direction of the wind, the Leeward Islands, *pro tempore*, became windward, or the wind blew from them to the southern or Windward Islands; for, though irregularly, the islands in general lie S. E. and N. E. not E. and W. Look at the chart and you will find that the N. E. wind blew over the vast Atlantic, and, from the extreme heat of day, must have been well saturated with moisture—satisfactorily accounting for the great dampness and coldness of the evenings, nights, and mornings. Had the wind blown S. E., we should *not have had a rheumatic* fever, as it would have wanted, from topographical causes, the immense quantity of Atlantic moisture with which it was loaded. Dr Stedman's stating its first appearance at the port nearest St Thomas, as proof of its imputation and subsequent diffusion by contagion, proves nothing,* as from the wind's prevalence at the time St Thomas was to *windward* of St Croix, and therefore Christianstadt was the *very place* where of necessity it ought to have first appeared. Is this observed in diseases notoriously contagious? The origin of most epidemics is involved in obscurity, and, till we are better acquainted with nature's laboratory, our opinions as to the source of particular epidemics must after all be little better than conjectural.

Both Mr Nicholson and Dr Stedman state that the epidemic prevailed at different seasons and states of the thermometer and hygrometer. Did not these gentlemen observe that the thermometric heat was intense during the day, and that the evenings, nights, and mornings, were cold, damp, and unpleasantly chilly? I have no means of knowing whether Mr Nicholson consulted his hygrometer in the evening, when I am convinced it would have indicated an atmosphere surcharged with moisture. Dr Stedman acknowledges the "heat was very great," but overlooks, or does not notice, the great coldness, dampness, and rawness of the nights, which always succeeded the intense heat of the day. This state of alternation of great heat by day, and dampness, &c. at night, prevailed in Antigua for some time before the epidemic broke out, and was its generating cause. Every unprejudiced reader, I think, will agree with me that the meteorological phenomena which I have mentioned are in themselves quite adequate to the production of the epidemic, without having recourse to unintelligible miasms or other occult causes, or even to the hurricane, suggested by my friend Dr Stedman. The latter occurrence has always been considered by the vulgar as having an opposite effect in these latitudes; and the old ones say, whenever an epidemic prevails, "they wish a 'brush'" (*i. e.* a slight hurricane) would occur, as it would be sure to render the place healthier. Popular opinions and maxims being founded on experience, are generally true, and I believe strictly so in the present case.

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* The wind during a great part of the epidemic veered between the points of N. and N. E. rendering St Croix the leeward island. In these latitudes northerly winds generally prevail towards Christmas and in the spring months.

The more enlightened we become, the more sceptical we shall be, in regard to the doctrine of contagion. No one doubts that lues venerea, gonorrhœa, and also variola, possess the power of contamination; but, as regards measles, erysipelas, scarlatina, and most of the exanthemata, one cannot be considered heretical if he does not altogether prostrate himself at the shrine of contagion. I am aware of the experiments of Dr Home. But I cannot regard them as *experimenta crucis*, since his patients were susceptible, and measles were raging at the time, and in all probability the *experimentees* would have had the affection, *rubeolated* or *not*. A lady in this part of the world having two children to send to England for education, and hearing that measles were more dangerous in a cold than warm climate, determined, if possible, to let them have the disease. Here she accordingly made them sleep with a person sick of measles during the person's indisposition, but they, to her surprise, escaped—were sent to England,—and the very first winter were both attacked with the disease, which was epidemic at the school to which they had been sent.

The alarming doctrine of marsh miasmata, as the cause of yellow fever,* appears to me not to rest on an immutable basis. I have been six years in Antigua witnessing all vicissitudes in atmospheric temperature and moisture, and every day seeing the town surrounded by swamps, notwithstanding which I have not witnessed a case of yellow fever, and have been informed by the most intelligent practitioners that black vomit has not been seen here since the epidemic of 1816, so ably * described by my respected and intelligent friend Dr Musgrave. Such being the posture of things, one's creed as to the marsh-growth of yellow fever must receive a severe shake, if not precipitation, from its hold. We hourly, daily, and yearly see the alleged cause in full operation, yet no effect is produced. Yellow fever seems to have left Antigua for some more genial clime. This is an endless theme; but I must stop here.

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* No one now, I believe, doubts the non-contagiousness of this fever.

There was no death in Antigua from the epidemic; nor did I bleed any patient, as it was not indicated in any case I saw, and I saw many, and some I daresay as severe as any witnessed in St Croix. The treatment was simple calomel and cathartic extract, followed up with the solution of the *sulphas mag.* with magnesia† in peppermint water. After the bowels were well opened, a light febrifuge mixture of sweet spirits of nitre, nitrate of potass, tartarized antimony, and colchicum was ordered every two hours, with occasional effervescing draughts, the pediluvium at bed-time, with 10 or 15 grains of Dover's powder. When pain of head was severe, cold applications were used with good effect. I never ordered a blister, as it never seemed necessary, nor did I hear that such a remedy was used in Antigua, though I can easily conceive that a case might have occurred rendering such application necessary. After the acute symptoms subsided, I found from 40 to 60 drops of the wine of colchicum, with 25 drops of laudanum, always insured a good night's rest, and relieved the patient much. Thirty drops of antimonial wine were occasionally added to the draught. In this fever, as I considered the inflammation was confined to the sheaths of muscles,—the harder or ligamentous capsular structures,—general venesection was not a remedy from which one *a priori* could have expected much benefit. How far local bleeding might have been advantageous I cannot say, as I never tried it, and leeches are not to be had in Antigua. The fever seldom lasted above 36 hours, but the secondary pains remained in some for weeks and months, and were always worse in the mornings, as from personal experience I can tell. Rubefacients seemed of much benefit in hastening the absorptive process, and removing the pains and thickenings. As in Europe, so here, some, at any change of the weather to damp and cold, are occasionally twinged by their old tormentor, to remind them that he still lingers, and that the enemy is not quite beaten from his fast hold.

Dr Stedman appears to have been highly successful by prescribing calomel followed by salts to open the bowels. Though this practice is sanctioned by several of the London Hospital physicians, I regret that my experience prevents me from placing so much confidence in calomel, which I maintain may be given repeatedly without properly evacuating the bowels, and with no other effect than that of producing slimy, dark, glutinous discharges, with griping. To have a full operation, the calomel should be combined with jalap or cathartic extract in suitable doses, and followed by salts in solution with minute quantities of *tart. antim.*, or the compound powder of jalap mixture may be used instead of the salt solution with good effect. With such medicine we may look for satisfactory evacuations, but never by giving calomel alone.

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* In one of the volumes of the Medico-Chirurgical Transactions of London.

† Magnesia was ordered from an idea of prevailing acidity in the *primæ viæ.*

I may state, that, for days before the fever appeared, several persons whom I saw complained of uneasiness in epigastrio, flatulency, &c. In my own person for days before I was seized, I experienced unpleasant epigastric distension, with frequent discharges of gas by eructation—in fact, symptoms of want of tone in the chylopoietic organs. I have no doubt, had minute inquiry been made, similar symptoms would have been observed in most persons before the febrile explosion. Pricking in the hands and feet were observed in many. Most of my readers are aware that the same occurs in persons labouring under deranged digestive organs, from what is called poisoning by fish. I knew a young lady here who was affected by eating a part of a king-fish, (*xiphias*,) and who suffered for a long time from articular pains, exactly such as occurred in the dandy. In my own case there was not the slightest efflorescence. I observed the same in a few others. My digestive organs being pretty good, the symptoms of their derangement before and during the attack were exceedingly slight, having had neither nausea or vomiting, anorexia alone indicating that all was not right.

The foregoing remarks have been hastily put together, literally *currente calamo*, on reading the observations of Messrs Nicholson and Stedman, which circumstance, I trust, will be a sufficient apology for want of order in the arrangement. It is not meant as a perfect sketch; and, should my observations induce medical men who have witnessed this truly distressing epidemic to give to the world their views of the disease, my obtruding myself will not have been in vain.

St John's, Antigua, 10th June 1829.

[Though it is our general practice to insert the papers of correspondents as we receive them, without comment or remark, and we are unwilling to deviate from this rule in the case of our friend Dr Furlonge, we think, however, we know that gentleman sufficiently well, to believe that he will not be offended by our stating that the arguments adduced in his paper are not adequate to prove the non-contagious nature of the West India eruptive epidemic. Whatever may be said of erysipelas, under certain circumstances, we believe, no good or satisfactory evidence has yet been adduced to disprove the contagious character of scarlet fever and measles. This is not the place to enter into any disquisition on the evidence sufficient to establish the contagious character of different diseases generally reputed so. But if Dr Furlonge limits this character to lues venerea, gonorrhœa, and variola, it is manifest that he requires much more rigid evidence than has been thought requisite by the generality of physicians, who have had occasion to observe the phenomena of the propagation of these diseases. Dr Furlonge will perhaps forgive us for reminding him of the distinction between contagion and infection established by Dr Adams, and the conditions required to demonstrate the propagation of infectious diseases delivered by that author.]—EDITORS.

ART. VII.—*Case of Tetanus Nascentium, successfully treated.* By JOHN FURLONGE, M.D., Member of the Royal College of Surgeons, and of the Royal Medical and Medico-Chirurgical Societies of Edinburgh.

MY object in publishing the following case is merely that of stating, as shortly as possible, the successful treatment of a disease considered invariably fatal, and in the sincere hope that the attention of my medical brethren in the tropics may be directed to more minute investigation of the etiology of the disease as it occurs within the fortnight. The known fatality of tetanus * occurring within the fortnight after birth, has, I am convinced, induced many to consider medical aid as useless; and consequently, in this part of the world, the medical attendant too often, I fear, contents himself with ordering the bowels to be kept open with oil, and the use of the warm bath twice or thrice a-day, instead, as in my opinion, of endeavouring to combat the affection by those means, which the best writers have recommended, and which are suggested by a review of the symptoms, and supposed pathology of the disease.

One principal reason, indeed, of the apathy of practitioners, in regard to *tetanus nascentium*, is the little noise or inquiry which the death of children within the fortnight after birth occasions; and besides, the medical attendant is not likely to suffer in his reputation. The proprietor, moreover, never having had the services of the deceased, makes little stir about its loss, and is quite satisfied, (from its known fatality,) if he learn that it died of locked jaw, as it is called.

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* I have called it *tetanus nascentium*, as I never saw a case of simple locked jaw; but invariably genuine tetanus, just as it occurs in adults from wounds or other causes.

I have seen eight or ten cases of this terrible affection, (*tetanus nascentium,*) and, with the exception of one, I have had the mortification to see my little patients consigned to their graves. I then determined to treat the next which fell under my care on principles precisely as if it were an adult.

The little patient in the following case was attacked on the eighth day after its birth with symptoms of trismus; it refused the breast; and was reported to have stiffness of jaws, for which warm applications were applied, and a dose of oil given.* I ordered calomel and jalap to open the bowels, and the warm bath every three hours. Early next morning I saw the child, which was not only *trismatic*, but universally *tetanic*. The little finger could not be introduced into the mouth; and the attempt induced considerable spasms of face and body, with moaning. The expression of face was tetanic,—an appearance never to be forgotten when once seen. I considered the case hopeless; and inserted in the hospital journal of the estate, that the child had locked jaw, and there was no hope of its recovery, having every reason for so thinking. I, however, ordered one drop of *tinct. opii* every three hours, and the warm bath four times a-day; the jaws to be frequently rubbed with warm laudanum; the place from which the navel-string had dropped to be dressed with turpentine and mercurial ointment; the nape of the neck and back to be blistered; and one of the following powders between each dose of the laudanum:

℞ *Pulv. Dover. g. ii. Sulph. Zinci, g. v. M.*

Next day I was gratified by a report, stating that the jaws were not so "tight as they had been," but that the "child was uneasy all night, bowels had acted, and blister risen well." Encouraged by this amelioration, I ordered to continue as before, adding a small quantity of camphor to each powder, the belly to be rubbed with warm laudanum, and the blistered surface to be dressed with *ol. terebinth.* and blue ointment. The same treatment was continued for three weeks. † The child during this period slept much, evidently from narcotic influence. It could not take the breast for a fortnight after its attack; but is now, four weeks from the first invasion of tetanus, free from every symptom of the disease, with the exception of very great tenderness of abdomen, pressure on which excites crying, and something like tetanic expression of countenance. It sucks and eats well. About the tenth day from its attack, it had a slight febrile affection, which I considered favourable. This

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[*] This was the report of the manager of the estate late at night. I saw the child next morning.

[†] Bowels opened with oil when necessary.

yielded to one grain of calomel, and one and a half of James's powder, and the warm bath. It took in all about three grains of calomel by the mouth.

Zinc was a favourite remedy in tetanus with a gentleman here, long since retired from practice, of which he enjoyed a very large share with unbounded public confidence. He informs me that he thinks zinc beneficial, from its emetic (nauseating?) property; but it is well known that it is with the utmost difficulty that vomiting can be induced (if at all) in tetanus. In my humble opinion, zinc acts as a powerful sedative, and, by repressing excessive action, becomes antispasmodic, *i. e.* by lessening the energy of the brain. I was led to this by the following fact:—I had occasion to order the *sulphas zinci c. ext. gentianæ* for a gentleman; and on accidentally putting my finger on the wrist, I was astonished at the slowness of the pulse. I felt alarmed, and asked if he had headach or fulness of head? He replied in the negative; shaking his head was not at all distressing to him. His pulse beat 48. It then occurred to me that the slowness of the pulse might be occasioned by the zinc. It was omitted for a day; and on the following the pulse rose to 72, its natural standard. The experiment was repeated, but irregularly, with, however, evidence sufficient to convince me that my conjecture was not unfounded. To enter into a discussion on the *modus operandi* of zinc would be carrying me into too extensive a field of inquiry, for which I have neither time nor inclination, my object being merely to state facts as they occurred, leaving for others to theorise if they please; but from what I have stated, the *modus agendi* of zinc in tetanus is obvious. Does the sedative effects of zinc on the heart arise from sympathy with the stomach, or by generally lessening the energy of the brain? I am inclined to the latter opinion. In the gentleman whose case I have mentioned there was not the least tendency to nausea. If I am not deceived in the effect of zinc, the practice of prescribing it in tetanus, which was considered empirical, is not void of philosophy, and is consonant with the soundest views of the disease. My friend, Dr Peddie, saw my little patient one day nearly three weeks after its attack, and pointed out to me the uneasiness and anxious expression of countenance excited by pressing the abdomen; and even at that time the fingers were still in a state of tonic spasm.

I have conversed with all the medical men here, and no one has ever seen recovery from *tetanus nascentium.* Dr Musgrave and Dr Peddie, who are both in very extensive estate and private practice, have never seen recovery take place. The same has been observed by Drs Odlum and Sedgwick, both also in very extensive estate and private practice. Is not the tetanic state directly in opposition to the state of syncope?* Tetanus is assuredly a curious and interesting, but very perplexing, disease, one of the opprobria of our art. I have sanguine hope, however, that we shall yet live to see tetanus more under the dominion of medicine. Dr Cullen, I am aware, recommends the oxide of zinc in epilepsy. I have seen it ordered without appearing to exert the slightest effect on the disease.

Trismus is never met with in the houses of the people of the first rank here, nor among free people of colour; and I am convinced that it might be altogether prevented if the following plan were rigidly adopted by managers and directors of estates. As soon as the child is born, the navel-string should be divided by a sharp scissors, and wrapped up in clean linen. Every night for a fortnight one or two drops of *gutta vitæ* † and castor oil should be given with a little magnesia every other morning to obviate constipation. The child must be bathed in tepid water every morning, and the funis dressed. No fire to be allowed in the lying-in room, or in the room next to it, which it will require the most persevering vigilance to effect. The rooms should be kept well ventilated, instead of which they in general represent so many stew-rooms. I have discovered that it is a common practice among the ignorant black midwives on estates, never to open the navel-string *till the ninth day from the birth*, and the children are not allowed to be put to the breast for several days. Can we wonder that locked jaw should carry off so many victims under so preposterous a system; but which I have broke in upon on those estates under my direction, it being a positive order to put the child to the mother's breast within the first twelve hours, and that the bath and dressing of navel, &c. &c. be a daily operation, on pain of punishment. I have upwards of 800 slaves under my charge, and have not seen locked jaw occurring in infants for eighteen months and upwards. Whether this is to be ascribed to the adoption of my system or not, I cannot positively tell, as medical men are never called to lying-in women on estates, unless some untoward circumstance occurs. My intelligent preceptor, Dr Hamilton, on reference to my notes, I find, advocates the propriety of giving laudanum to infants as a preventive, with a view to its diminishing susceptibility. Whether smoke has anything to do with the generation of trismus I cannot take upon myself to state, but we know it occurs in the Highlands of Scotland, where it is a very common practice to have peat fires in the houses.

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* Notes of the above were written more than six months ago. I have within the last month seen the Edinburgh Medical and Surgical Journal for January 1829, in which are inserted some observations of M. Doucet, from the Nouvelle Bibliothèque Médicale for March 1828, on the cold effusion in tetanus, in which it appears that its beneficial effects were always proportional to the state of syncope induced. So far this is confirmatory of my query. M. Doucet does not attempt to explain its *modus operandi*, nor does he state his views in prescribing it.

† What is called *gutta vitæ* here, is a mixture of equal parts of tincture of opium and spirits of wine, kept on the estates ready for exhibition.

St John's, Antigua,
29th June 1829.

ART. VIII.—*A Case of Poisoning by Arsenic.* By HUGH WARD, M. D. Dumfries.

JAMES MACINALLY, æt. 17, tailor, on Saturday, 3d October 1829, about 7 A. M. after a night's debauch, and about seventeen hours after any food was known to have been taken, mixed with water in a tin vessel four drachms of arsenic, which he had procured about two weeks previously. He drank about three-fourths of the poison, (one-fourth having fallen to the bottom of the vessel,) and went to bed to sleep. It was not until two hours after this discovered that he had taken poison. Some time was lost, the persons about him not believing he had done so, as he was apparently in his senses, and to their knowledge had no cause to commit so rash an act, having only received a reproof from his foster-mother for keeping irregular hours and indifferent company. But he was naturally of an irritable and stubborn temper, which may partly account for his committing such a deed. About half-past 9 A. M. being summoned to attend, I found him in bed disposed to sleep, with the eyes suffused and languid; the pulse 90, soft, and rather feeble; and the tongue much furred. When asked what was wrong, he said he merely wished to have a sleep, and then would be well. He had slight nausea, but complained of no pain, except slight tenderness in the region of the stomach when firmly pressed. The action of the heart was natural. I left him, in order to procure an emetic, which I intended to give him, should he not vomit, with tepid water, which I directed him to drink. On returning in ten minutes, I found that he had vomited in my absence a quantity of mucus and saliva, in which was mixed a portion of the poison taken, to appearance about a drachm. He continued the use of the tepid water to promote vomiting, which he drank freely and ejected as freely; and he was directed always to place himself in the horizontal position when inclined to vomit. Drinking and vomiting were continued with little intermission till half-past 12 noon. He was left about half an hour with instructions to drink half a quart of the decoction of lintseed every ten minutes, which he did, and vomited twice during my absence. About 1 P. M. the pulse was increased in frequency to 112, but not in strength, although I now observed that the heart had begun to act with much apparent energy. At 2 P. M. he was again left, with instructions to drink the above quantity every five minutes, which he did, and during my absence he vomited three times.

At 3 P. M. pulsations of the heart strong and sharp, pulse at the wrist 116, sharp, but easily compressed. Judging the case to be a desperate one, and finding his strength failing, I requested a consultation, for which purpose my friends, Messrs M'Ghie and M'Lauchlan were called in. In consultation it was judged proper to wash out the stomach with a solution of some alkaline substance. Two ounces of the carbonate of soda were dissolved in about a gallon of tepid water. The stomach-pump was in readiness, but as he drank and vomited very freely, it was not used.

Upon examining from time to time what he vomited, some small particles of the poison were discovered either detached or suspended in mucus, except in the last ejected, in which none could be detected by the eye. Altogether he drank and vomited upwards of five gallons of fluid; and I may observe, that the whole of it, when ejected from the stomach, had a slightly yellow tinge, and no food was discovered in any of it. Not doubting that some of the poison must have passed from the stomach into the bowels, three quarts of warm gruel, with a piece of butter, were given as an enema to shield the bowels from the action of the poison, as well as to procure a motion. Though he only complained of a slight tenderness over the lower edge of stomach and around the umbilicus, when firmly pressed, (I say firmly, because slight pressure gave no uneasiness;) yet, as inflammation was to be dreaded, we thought proper to apply sixteen leeches over the tender part, to be followed with warm poultices to encourage the bleeding, and his drink to be two ounces of the sweetened decoction of lintseed every half hour.

8 P. M. His appearance improved, eyes more clear, and face less sallow; pulse 118, soft; action of heart still violent. Leeches have been applied. Has vomited several times; inclines to drink. The decoction of lintseed was directed to be continued at longer intervals, and in smaller quantities. He was also instructed to rinse his mouth with cold water occasionally, and to have an ounce of castor oil, one-half of which to be taken at present, the other four hours after.

10 P. M. Much the same; pulse 115, and soft. Continue former treatment.

12 midnight. A little restless; pulse as frequent, but more soft; action of heart not so strong. Says he feels a craving for food. Countenance less animated; eyes more languid; evidently

not so well as at last visit. Has not vomited. Poultices over leech bites to be discontinued; oil not to be given till two in the morning; and a table spoonful of gruel to be given every half hour, instead of the decoction of lintseed. At 3 in the morning, my friend Mr M'Ghie was called up to visit him; and found him very restless, with feeble and rapid pulse, sunk countenance, and cold extremities, but without any increase of pain. He was apparently sinking. The second dose of castor oil had been vomited. Mr M'Ghie gave him a draught consisting of camphor, hyoscyamus, and mucilage, and directed heat to be applied to the extremities.

Half-past 7 morning.—Says he is much better; has very little pain in the epigastrium on pressure; is rather more thirsty; vomited the draught about half an hour after it was given him in the night; he has vomited several times since. Gruel to be continued.

11 A. M. We called Dr Symons in consultation. Pulse 115, soft; action of heart still strong. Says, when asked, that he feels a little pain in the stomach, but this only when pressed; thirst increased. He now dislikes the gruel and the decoction of lintseed. Has not vomited since 9 A. M. Twelve leeches to be applied over the stomach, and blood to be extracted with cupping-glasses, taking the pulse for a guide as to quantity. To have milk or milk and water to drink *ad libitum*. Extremities to be rubbed with camphorated spirits. A double portion of camphor to be added.

1 P. M. Leeches have been applied. Could not suffer the application of the cupping-glasses from the pain which they caused; has not lost more than an ounce and a half of blood. Pulse 115, more feeble, and now becoming irregular; action of heart diminishing also. A blister to be applied over the stomach extending to the umbilicus. The camphorated spirits to be continued.

4 P. M. Very restless; countenance anxious; eyes more suffused. Says the blister gives him much pain, and desires it may be removed. Pulse 115, very feeble; action of heart not so strong; skin covered with a cold sweat; extremities cold; bowels have since 2 P. M. moved several times, and he has still an inclination to go to stool; stools rather dark coloured and bilious, mixed with a little mucus. Blister being removed, says he feels easier.

7 P. M. Much worse; more restless, and cannot tell from what cause. Thirst considerable; pulse almost imperceptible; action of heart very feeble and irregular; complains of burning heat over the body, though the skin to the touch feels cold; body covered with profuse cold perspiration.

Half-past 9 evening. Constant tossing in bed; countenance exceedingly anxious; frequent sighing, but cannot say from what cause. The pupils are insensible to light; pulse imperceptible; action of heart can scarcely be felt; bowels still moving very frequently; stools have a brownish colour mixed with mucus; still answers questions distinctly; thirst considerable; can lie on the belly and endure pressure over it. He continued gradually to sink till death put an end to his mortal career a few minutes past 12 midnight,—forty-one hours after taking the deadly draught.

No blood appeared in what he either vomited or passed by stool. There was no pain, heat, or constriction in the gullet. There was no pain or heat in the stomach complained of; slight pain there was only felt on pressure. The thirst was not urgent,—in fact, the prominent symptoms of poisoning by arsenic were absent.

The inspection was performed fourteen hours after death in presence of several medical friends. Features placid; face pale; a yellowish fluid escaping from the mouth; extremities exceedingly rigid; muscles generally much firmer than usual after death. A little livid discoloration under each ear, extending around and down the neck to the top of the chest with the scrotum, glans penis, and nails. On laying open the chest and abdomen, the muscles, which were divided, presented a fine bright red appearance. Pleura generally more vascular than usual, particularly that portion of it which is reflected over the diaphragm. The left lung, from old adhesions, was adhering to the pericardium. Little fluid in the cavity of the chest. Pericardium very muscular, and containing about two ounces of reddish-brown fluid. Heart rather smaller than natural, and hard like a muscle when spasmodically contracted; external surface natural, but when cut into, it presented the unusually red appearance which the other muscles possessed. Scarcely any blood in the left ventricle; the right appeared to be full; the coagula found in the ventricles were exceedingly fragile. Lungs generally of a deep bluish purple; no collapse of them; they wholly filled the space allotted to them in the chest, and when cut into their vessels were found gorged with black blood. Omentum very vascular, but not adhering. Small intestines distended externally; much more vascular than usual; their vessels presented a beautiful arborescent appearance. No adhesions between them, nor any appearance of coagulable lymph having been thrown out. Stomach externally more vascular than natural, particularly along its larger curvature and about the pylorus, where there were several patches of the size of a shilling of a deep red or livid colour. On opening the stomach there was found about twelve ounces of a thick yellowish fluid; its inner coat generally more vascular than natural, especially along the larger curvature and about the pylorus, where there were several thick corrugated patches of a deep livid colour. There were also several spots, each about a line or a line and a-half in diameter, of a brownish-yellow colour, as if the particles of some substance were engrained into the villous coat. Pylorus much thickened and very vascular. Liver externally much paler than usual, but when cut into of its usual appearance. Gall-bladder partly filled with bile of a saffron colour. Colon rather more vascular than usual, and dilated from the *caput coli* to the sigmoid flexure, where it became much contracted as far as the rectum, not thicker than the thumb, and firm; when opened not much more vascular internally than usual; where contracted, its parietes were much thickened, and the ligamentous bands of the whole of it more contracted than natural. Small intestines when laid open less vascular internally than externally, but their rugæ very distinct and firm. A thick yellowish fluid free from fæculent smell found in the small intestines, but little in the large. No abrasion or ulceration of the internal coat of the stomach or intestines was perceived. Spleen of a darker hue than natural. Kidneys more vascular than usual; in the pelvis of the left there was a small quantity of a purulent fluid. Bladder much contracted and firm, not larger than a winter pear; its internal coat more vascular than natural, and its rugæ very distinct. The mesentery considerably injected. A little effusion into the abdominal cavity.

OBSERVATIONS BY DR CHRISTISON.

Having, through the kindness of Dr Ward, had an opportunity of examining the stomach and its contents in the preceding valuable case, the following additions to his account of it have appeared to me useful for completing the narrative, and are made with his permission.

The contents consisted of a thick, yellowish-white mass, without any blood. In the bottom of the bottle which contained it there was a grayish heavy sediment. This was separated by decanting the superincumbent fluid, and then washed with cold water. Pure water being next boiled over the sediment, this was dissolved almost entirely. The solution gave a copious gamboge-yellow precipitate with ammoniacal nitrate of silver, a considerable apple-green precipitate with ammoniacal sulphate of copper, and an abundant sulphur-yellow precipitate when acidulated with acetic acid, and treated with sulphuretted-hydrogen gas. The greater part of the solution having been reserved for the last experiment, about a grain and a half of sulphuret was procured. The fluid part of the contents was next boiled and filtered; and the pale straw-coloured liquid which passed through the filter was treated with acetic acid and sulphuretted-hydrogen gas, upon which a quantity of sulphuret was procured, amounting to almost as much as that obtained from the sediment. A portion of this was subjected to the test of reduction with black flux by one of my pupils, who thus formed the usual brilliant crystalline crust.

Not long ago it would have been accounted a remarkable circumstance, that so much arsenic should remain in the stomach after frequent vomiting, and the maintenance of life for one and forty hours. But late investigations on the subject show that this is not at all unusual, and that the poison will probably be detected, although the patient should have vomited for two or even three days.

The brownish-yellow dust described by Dr Ward as covering the inner coat of the stomach was, when I examined it five days after death, bright sulphur-yellow, and manifestly sulphuret of arsenic formed by the action of the oxide with sulphuretted-hydrogen disengaged after death. This remarkable appearance I have now witnessed four times, so that, although hardly ever mentioned in the accounts of cases, it must be common.

The villous coat in this case was perfectly entire and firm. The "thick corrugated patches of a livid colour," described by Dr Ward, presented a very striking appearance. The rugæ were here and there elevated nearly to the size of half the little finger slit longitudinally: these elevations were dark-bluish-black, hard, and incapable of being extended when the peritonæal coat was stretched; and they were formed by blood minutely extravasated and incorporated with the cellular tissue between the villous and muscular coats. This state of the villous coat is in all likelihood the early stage of an appearance which I have witnessed on a former occasion, and described in this Journal, Vol. xxvii. 453. The rugæ in that case were some of them in the state now described; but on the surface of others the membrane had given way, and there the surface of the black patches consisted of intermingled blood and mucous tissue. I am much mistaken if either appearance is ever produced by natural causes. *Pyl*, in his Essays and Observations on Medical Jurisprudence [*], has mentioned a case where the rugæ were in the same state as in the instance related above by Dr Ward; and *Metzger* [†] has described another. The appearance, therefore, is not very uncommon.

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[*] Aufsätze und Beobachtungen aus der gerichtlichen Arzneiwissenchaft, i. 58.

[†] Schlegel's Collectio Opusc. ad. Med. Foren. Spectantium, iv. 23.

Art. IX.—*Cases and Observations in Medical Jurisprudence.—V. Poisoning with Arsenic.—VI. Poisoning with Opium.—VII. Poisoning with Elder Flowers and Leaves.*

By Robert Christison, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c. &c. (Continued from Vol. xxxi. p. 250.)

In the present paper, which is a continuation of those formerly published in this Journal, I propose first to describe the particulars of an alarming accident which lately happened, in consequence of a bottle of Champagne having been impregnated with arsenic. I shall next notice a case of poisoning with opium, which will pointedly illustrate the extreme difficulty of procuring, in cases of poisoning with this drug, satisfactory chemical evidence. And, lastly, I shall relate an example of a very rare description of poisoning, which lately came under my notice, arising from the leaves and flowers of the elder-tree having been eaten by a little boy.

Case V.—*POISONING WITH ARSENIC.—Simultaneous poisoning of six persons.—Taste of Arsenic.—The Test of Reduction may supply evidence when the quantity of metal sublimed is less than a 250th part of a grain, and is too minute to form a characteristic crust.*

On the 1st November the family of a Baronet in Roxburghshire, together with several visitors, amounting altogether to six persons, all adults, were taken severely ill during dinner, or soon after it. Most of them rose from the table before the cloth was removed; but two of them were not seized till about an hour after the rest. The symptoms were in all sickness, vomiting, and severe pain of the bowels; and in no long time one of them had also diarrhœa. The vomiting continued violent till early next morning; and warm water was taken freely to encourage it. In the course of the night all were affected with a sense of heat in the stomach, throat, and mouth; and in the morning the lips became encrusted, and the skin cracked and peeled off. For three or four days the whole party had a disinclination to eat.

The simultaneous occurrence of such symptoms, during a meal, in so many persons, left hardly a doubt that some poisonous substance had been mixed with one or other of the articles used at dinner. In order to discover what the poison was, and in what vehicle it had been taken, various articles were sent to me for analysis through Mr Stuart of Kelso, one of the surgeons who attended the family.

It was at first imagined that the only article of which the whole party had partaken was soup, to which my attention was consequently directed in the first instance. In a general search for poison, it appeared to me that where no particular substance was pointed at, and the taste did not indicate an admixture of any of the vegetable poisons which could cause symptoms like those specified above, the most natural substance to search for, and the only one which there was any probability of detecting, was one of the common metallic poisons. With this view, after filtering a portion of the soup, and acidulating it with acetic acid, I passed sulphuretted-hydrogen gas through it for fifteen minutes. But no change of appearance was induced. In this way it was rendered quite certain, that the soup did not contain any preparation of arsenic, mercury, copper, antimony, lead, or zinc. As the taste of the filtered soup was perfectly good, it was not easy to suppose that any other poison existed in it.

The next article examined was the vomited matter. Two quarts of this were sent me. It was taken from a pail which contained about four gallons,—the matter vomited by four of the party. A small portion of this when filtered gave no indication of any of the above metallic poisons on being treated with sulphuretted-hydrogen. But when the whole filtered fluid was evaporated to the volume of two ounces, sulphuretted-hydrogen produced a dirty yellowish cloudiness, which, after ebullition and subsequent rest for twelve hours, gave place to a scanty, Naples-yellow, flaky precipitate. This precipitate was separated and washed by the process of subsidence and affusion repeatedly performed, and was then dried in a watch-glass. The product, which was very small in quantity, and of course contained a large proportion of animal matter, was then subjected to the process of reduction in one of the small tubes recommended by Berzelius. By slow and cautious management of the heat, a scanty sublimate was procured, forming a dark, filmy cloud, on a small part of the narrowest portion of the tube. This sublimate was entirely destitute of brilliancy on the outside, or of crystalline appearance on the interior; and consequently without the subsequent test of oxidation, suggested lately by Dr Turner, no conclusion whatever could be drawn from it. But on removing the portion of the tube containing the flux, and then subjecting the film to repeated sublimation, a ring of fine sparkling white crystals was formed, on some of which I could observe with a common magnifier triangular facettes. I could hardly doubt, therefore, that I had procured a minute quantity of oxide of arsenic. But as the quantity was so small, that, being in the custom of weighing somewhat larger quantities, I was sure it did not amount to nearly a 250th part of a grain, I resolved to subject it to a farther test. With this view two drops of water were introduced into the tube, and boiled on the crystals. These were soon dissolved, and in the solution the ammoniacal nitrate of silver caused as characteristic a lemon-yellow precipitate as it could produce in any arsenical solution.

The only obstacle in the way of the conclusion, that arsenic existed in the vomited matter, now was the possibility of a trace of it obtaining admission from the vessels employed in the analysis; for as these vessels consisted of four large glass funnels, as many precipitating glasses, and a porcelain evaporating basin, it might with some show of reason be said, that a few specks of arsenic scattered up and down these vessels, and too small to be detected by the sharpest eye, might be sufficient to make up the quantity procured. This consideration certainly led me to hesitate a little in pronouncing that arsenic had originally existed in the subject of analysis. At the same time there could be no doubt that the surface of the funnels and precipitating glasses, as well as the porcelain glaze of the evaporating basin, would, when clean and dry, enable one to detect the merest speck of adhering powder.

Fortunately all reason for hesitation was removed by the subsequent proceedings. It was afterwards remembered by the party that they had all partaken of certain bottles of wine, the remains of three of which were therefore sent to me. In a bottle of Teneriffe, and in another of a light French white wine, sulphuretted hydrogen gas caused no change whatever; and both of them were free of foreign taste. But in the remains of a bottle of Champagne, which also was free of any taste except that of vinous sweetness, the same test caused a copious sulphur yellow precipitate. The arsenical nature of this precipitate was proved by the process of reduction. Two ounces of the wine gave one grain and a quarter of sulphuret of arsenic, corresponding to one grain of oxide of arsenic.

On subsequent inquiry, it appeared that the bottle of Champagne was brought from the cellar before dinner by the baronet himself, who undid the wire during dinner, immediately before the wine was used. This circumstance at once showed that the wine had been poisoned before the bottle was corked by the wine-merchant.

Each of the six individuals took a single glass of it, or about two ounces; and consequently each took a grain of arsenic. It has been generally said in systematic works that a grain may prove dangerous. But I am not aware that any direct and positive fact has hitherto been published in support of this statement. The severe effects experienced by all the individuals in the present instance, notwithstanding that the poison was not taken till the stomach contained a considerable quantity of food, proves that on an empty stomach a single grain of arsenic might cause very alarming symptoms, if not even death itself. —Four of the party were taken ill about ten minutes after drinking the wine, which is a sufficient answer to those who imagine that arsenic never begins to act in less than half an hour.—None of the party perceived any unusual taste in the wine. This is a new and quite impartial testimony on the important question of the taste of arsenic, concerning which so many vague and erroneous statements have been published by authors and sworn to by witnesses *. In conjunction with other late instances of the same purport, it establishes the fact, that arsenic may very readily be swallowed in considerable quantity in articles of food without its presence being suspected.

I shall conclude by referring again to the analysis of the vomited matter, and requesting the attention of chemists and physicians to the extreme delicacy of the process of reduction as a test for arsenic. I have already made public several statements on this subject; but in no instance have I ever detected so small a quantity as on the present occasion. The experiments detailed above prove that the process of reduction may furnish satisfactory evidence of the presence of this poison, when the quantity sublimed is too minute to form a characteristic crust in its *metallic* state.

CASE VI.—*POISONING WITH OPIUM.—Process for detecting Opium in mixed fluids.—Uncertainty of the chemical analysis.*

Mary White, aged 20, a servant, was admitted on the 8th July 1828 into the clinical wards of the Royal Infirmary under the care of my colleague Dr Alison, to whom I am indebted for the following history of her symptoms.

She was reported to have swallowed two ounces of laudanum at half-past seven on the morning of her admission. About half-past ten she was found in bed in a state of deep sleep. At eleven an emetic was given, and its operation assisted by draughts of hot water; but she vomited only two mouthfuls of clear liquid, which was said to have had the smell of opium. At half-past eleven she was brought to the Infirmary, when she presented the following symptoms.

The countenance was pale and languid; the eyelids drooping; the pupils in a state of contraction; the vision distinct. She answered questions pertinently, fluently, and willingly, but was very anxious to be allowed to go to bed, and had the greatest inclination to fall asleep. She could swallow easily. The pulse was 86 and not strong, the tongue dry and brown, the respiration faint, the skin cold, and the general sensibility somewhat impaired. She complained of headach and giddiness, but had no pain in the pit of the stomach. The stomach-pump was immediately resorted to, and three basinfuls of water were injected and withdrawn. The first portions had the smell of laudanum. About five pounds contained in the first basin were subsequently subjected to analysis. After the stomach had been thoroughly cleared out, brisk cathartics, lemonade, and coffee were ordered, and she had the tepid affusion. At the same time directions were given to keep her in constant exercise in the green of the hospital. At four in the afternoon the pulse was only 48 and full. At seven the drowsiness had nearly gone off, and the skin was warm. The forced exercise was therefore discontinued. She slept quietly the whole succeeding night.

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* See my *Treatise on Poisons*, lately published, p. 175.

Next morning she complained of headach, and the face was flushed, and the pulse 84 and full. She had some vomiting and several scanty stools. There never was any itching of the skin, which M. Bally has lately considered an invariable symptom of poisoning with morphia and its compounds. * Purgatives were again prescribed, and the tepid affusion was repeated. In the evening, as her headach had increased, ten leeches were applied.

On the morning of the third day the headach was gone; but she complained of pain in the pit of the stomach, increased by pressure and coughing. The pulse was rather slow, the tongue furred, the bowels open, and the drowsiness not entirely removed. Purgatives were again ordered, and effervescing draughts given from time to time. Next morning, the 11th, she was quite well, and left the hospital.

Chemical Analysis.—The contents of the first basin, which had a distinct smell of opium when first withdrawn from the stomach, lost it almost entirely after exposure to the air for an hour. At least when I first examined the fluid after that interval, I was not sensible of any other odour, except the usual sourish, heavy smell of the natural contents of the stomach.

The fluid was colourless and nearly clear, and it contained only a few strings of mucus. When filtered, and cautiously evaporated to the consistence of a thin syrup, it was treated with boiling alcohol. A large quantity of grayish flakes was thus thrown down, and the alcoholic fluid after filtration was brownish-red, clear, and distinctly bitter. This solution was in its turn evaporated to the consistence of a thin syrup, and then treated with distilled water. More insoluble matter was thus separated, and a fluid was procured of the same sensible properties.

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* Mém. de la Soc. Roy. de Méd. 1827.

This fluid was next precipitated by an excess of acetate of lead. After filtration and the subsequent removal of the excess of lead by a stream of sulphuretted-hydrogen, a very pale straw-coloured liquid was procured, which still retained a distinct bitter taste. This, when gently concentrated, precipitated with ammonia; and the precipitate formed slowly in tufts of radiated capillary crystals of a pale-brown colour. These crystals when cleaned with a little cold water, dissolved freely in boiling alcohol, and crystallized again after the alcohol had nearly all evaporated. They were so minute in quantity, that I could subject them only to one of the tests for morphia, nitric acid, which immediately dissolved them, forming a bright yellow fluid.

The insoluble matter thrown down by the acetate of lead was washed on the filter, and suspended in distilled water, through which a current of sulphuretted-hydrogen was transmitted. In this way the whole lead was thrown down in the form of sulphuret of lead, and all acids contained in the precipitate formed by the acetate of lead were disengaged and dissolved. With the view of discovering whether any meconic acid was thus separated, the solution was filtered and evaporated to the volume of half an ounce; upon which a faint cherry-red colour was struck in it by the perchloride of iron.

Observations.—My principal object in publishing the case now described is to show, in opposition to the expressed belief of some medical jurists of authority, that even in circumstances apparently very favourable, the analyst may fail to procure satisfactory indications of the presence of opium in cases of poisoning with that drug.

In this instance two ounces of laudanum, a quantity rarely exceeded in cases of poisoning, was retained only four hours in the stomach. Very little could have been discharged before the stomach-pump was used, as appeared from the want of colour of the small quantity of matter previously vomited. To the first portions of the contents of the stomach I applied the process recommended in my Treatise on Poisons,—a process which I have found to be one of much delicacy, and which is derived in part from the instructions I received from *M. Robiquet* of Paris, when I was his pupil, and partly from an analogous method devised by *Lassaigne* for detecting acetate of morphia. Nevertheless, I was unable to develope any of the characters of morphia except its bitterness, its precipitation from a state of solution by ammonia, and the imperfect action of nitric acid, which with pure morphia produces an intense orange-red solution; and the only proof which could be procured of the presence of meconic acid was the imperfect action of the perchloride of iron, which with even a very minute trace of pure meconic acid occasions in the solution an intense cherry-red colour.

Supposing the preceding case to have been a criminal one, would these facts have constituted sufficient evidence of the presence of opium? I must confess I have some doubts on the subject. If, however, I had been quite satisfied of the opiate odour having existed in the fluid drawn from the stomach, my mind would have been made up; but no odour of the kind was perceptible to my sense of smell an hour after the contents were withdrawn; and the sense of smell is so apt to lead to error, that no sound inference can be drawn from it, unless several people agree in opinion as to the odour. The circumstances of the case show the propriety of applying the test of the odour immediately after the contents of the stomach have been removed.

It is hardly necessary to add, that the failure of the chemical analysis in such cases is owing not so much to imperfections in the process, as to the rapid absorption and decomposition of the poison in the stomach.

The present is not the only occasion on which my attempts to discover opium, in cases of poisoning with it, have failed. But it is unnecessary to state the particulars of any others, as they present no peculiarity. The subject is now becoming one of very great importance in medical jurisprudence. Many examples of poisoning with this drug have occurred where it was given for criminal purposes, either with the direct intention to murder, or for the purpose of causing stupor previous to the commission of robbery; and since the recent trial of Stewart and his wife before the Justiciary Court here for these crimes, the latter crime has become apparently much more frequent than ever. It is of great consequence, then, either that a delicate and certain process be devised, or at all events that the medical jurist know the exact extent of his powers.

CASE VII.—*POISONING WITH THE COMMON ELDER.—Enteritis produced by the leaves, and narcotic [illegible] the flowers.—Fragments of the leaves discharged [illegible] days after they were swallowed.*

of the 17th May 1828, two boys, while
neighbourhood of Edinburgh, ate, one
other the flowers and leaves of the

common elder. One of them, William Ross, eight years of age, was well enough when he returned home about two hours afterwards. But during dinner, which he took immediately on his return, he began to complain of severe griping. In the evening the griping still continued, he became also hot and feverish, and he had excessive tenderness over the whole belly. Next morning the symptoms were unabated, and in the course of the subsequent day and night the tenderness of the belly became so great that he could not allow it to be touched, while the tormina recurred with violence every half hour. He had also soreness of the mouth and throat. On the morning of the 19th he was for the first time attacked with vomiting, which returned frequently in the course of the day, and towards evening was tinged with blood.

His mother being then for the first time told that he had eaten some leaves on the 17th, sent for a surgeon on the morning of the 20th, who gave him calomel and three successive injections to move his bowels, but with hardly any effect. The symptoms on the evening of the 20th continuing unabated, she carried him to the Royal Infirmary here, where he became my patient.

When he was first admitted, the belly was very tense, much swelled, and exceedingly tender. The pulse was frequent and small, the face flushed, the tongue clean. He was immediately ordered an ounce of castor oil, an emollient injection, twenty leeches to the belly, the warm-bath, and constant fomentations. Much relief was derived from the leeches and the warm-bath; and next morning the swelling and tension of the belly were considerably diminished. The bowels not having been moved by the castor oil, two doses of ipecacuan were ordered, which caused both vomiting and several alvine discharges. The matter discharged was unfortunately not kept by the nurse. When I first saw the boy the same day at the visit hour of the hospital, I found the belly still tense and excessively tender; and he was affected with occasional hiccup and constant pain in the bowels, varying, however, in severity. The pulse was 140 full, and rather sharp; the tongue furred on the centre, and the skin rather hot. A large blister was immediately applied over the upper part of the belly, the warm-bath was repeated, an emollient injection was administered, and directions were left to take blood from a vein in the evening, if the symptoms should not be much mitigated. But he was so much relieved by the bath that venesection was considered unnecessary. The hiccup ceased, and the griping became much less frequent.

Next morning, the 22d, I found that although he had passed a restless night, owing to the blister, yet the pulse was 108 and soft. He had passed two copious stools, consisting of broken down fæces, mixed with a considerable quantity of small fragments of leaves. The fragments were so small that the plant to which they belonged could not be recognized; but the boy's companion brought me a branch of the tree from which the leaves they ate had been taken; and it proved to be a branch of the common elder. The warm-bath was directed to be repeated whenever the tormina were troublesome, and the emollient injection was also again administered. On the 23d the griping occurred very rarely. He had passed five loose feculent stools, in which fragments of leaves were no longer to be seen. During the two subsequent days he had frequent loose stools without medicine. All the symptoms continued to abate. On the 27th the stools were becoming again firm, and no pain was felt in the belly on firm pressure. The pulse, however, continued above 100, and the skin hot. On the 1st of June the stools were natural in consistence, and his complaints were all gone except weakness. As his parents wished to take him home, he was dismissed.

The other boy, Thomas Paterson, eleven years of age, ate the flowers only of the same tree by which Ross had been poisoned. During the remainder of the day he had no complaint; but in the course of the night he had violent headach; next morning he was very giddy when he rose; and in the forenoon the giddiness increased so much, that he staggered in walking, like a drunk man. A laxative administered at this time caused both vomiting and purging. Nevertheless the headach returned violently at night, and on the morning of the 19th he was still so giddy that he could not walk. In the course of that day he got gradually better, and next morning was quite well. He never felt any pain, never had any sickness except when he took the laxative, and took his meals all the time as usual.

Observations.—I believe the Common Elder, or *Sambucus Ebulus*, is not usually considered so deleterious as it appears to have been in the former of these two cases. The berries are well known to have a tendency to cause vomiting, though many people can eat a moderate quantity without that effect. The *Sambucus nigra*, or dwarf elder, is generally understood to possess rather active irritant properties. *Withering* says the inner green bark is strongly purgative, that the leaves are equally so, and that the flowers kill turkeys *. *Linnæus* states that the berries kill fowls and the flowers peacocks †. *Murray* observes that the inner bark is a drastic purgative[*]. *Bartholinus* mentions that some people who ate the leaves and tender shoots with vinegar and oil as a sallad, were attacked with violent purging to the extent of forty times in two days, and that a comatose state ensued on the third day[†]. Sommer, a writer in the German Ephemerides, likewise mentions two cases where excessive vomiting and hypercatharsis were induced, and great debility and emaciation for some time afterwards[‡]. It is not improbable that both species possess the same properties, but the *S. ebulus* in a less degree than the *S. nigra.*

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* Botanical Arrangement, ii. 426. Sixth Edition.

† Flora Suecica, p. 97.

ART. X.—*Case of Injury of both Hemispheres of the Brain.*

By HENRY MAUNSELL, Licentiate of the Royal College of Surgeons in Ireland, and Medical Superintendant of the Letterkenny Dispensary.

ON Saturday, November 22, 1828, I was requested by Mr Hunter of Letterkenny to visit with him William M'Elwee, son of a farmer residing about a mile from the town, under the following circumstances.

The patient, a stout young man, 22 years of age, had been discharging a yeomanry musket rather overloaded. In the explosion the breech of the gun was driven forcibly from the barrel, and its direction being changed by the resistance of the screw that attached it to the stock, the flat extremity of its cylindrical portion struck the middle line of the frontal bone a short distance above the level of the superciliary ridges, and, penetrating into the substance of the brain to the depth of an inch and half, carried with it a piece of bone three-fourths of an inch in diameter, which was exactly its own size. The shoulder of the breech, behind the cylindrical portion, being an inch and quarter in breadth, the bone, on either side of the circular wound, was irregularly broken, so as to admit of its passing through, and the brain of course correspondingly injured. A short time after the shot had been fired, he was found by his father, fallen forward against the wall on which he had rested the gun, but perfectly sensible. Portions of brain and some blood were sprinkled on the stones in the vicinity. The father and another person who was present, assisted by the patient himself, made several attempts to remove the piece of iron from the wound, but without success. They continued their efforts, however, for an hour, and often, as they informed me, with force sufficient to have raised half a hundred weight.

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[*] Apparatus Medicaminum, iv. 18.

[†] Acta Hafniensia, i. 164.

[‡] Ephemerides Naturæ Curiosorum Dec. ii. Ann. ix. p. 48.

About an hour and a half after the accident Mr Hunter arrived, and, by using some address, soon removed the foreign body. A cavity was then exposed, into which he introduced his little finger as far as the second joint, and, having felt the piece of bone, he extracted it by the forceps, drew the flaps of skin together, and placed a poultice over the forehead. At this time the surface of the patient's body was cold and his pulse weak; he was, however, perfectly sensible, and did not complain of pain. After the dressing vomiting came on, which produced a slight discharge of blood. At 5 o'clock P. M., about five hours after the occurrence of the injury, I saw him. Reaction had evidently commenced; his skin was warm; the pulse 78; and he had headach. The brain had risen into the opening, and could be seen pulsating strongly; small portions of it were smeared over the face and eyelids; and blood was trickling from the nostrils. He was bled to ten ounces, which sensibly affected the pulse; a light fold of lint wet with cold water was substituted for the poultice, and perfect rest and abstinence enjoined. It was remarkable that the use of the lancet appeared to give very great pain.

23d. Had a quiet night, sleeping tranquilly. He expressed much relief whenever the dressings were wet with cold water, which was done frequently. Can see distinctly; passed urine once, but has had no alvine evacuation; tongue clean; pulse 78, strong. Venesection to twelve ounces. *Capiat bol. ex scammon. et cal. abrad. capilli.*

24th. Rested pretty well; medicine has not operated; complains of some pain in the head, but not at the seat of injury; pulse 68, soft, and natural. Long fibrous portions of brain resembling portions of a nerve are seen issuing from the wound. *Mist. purg.* Evening. Pulse 72. Venesection to fourteen ounces.

25th. Continues as reported yesterday. Was purged once Head nearly free from pain; pulse 68. Continue cold applications.

26th. Was freely purged. Slight pain of head; skin rather hot; pulse 64.

℞. *Tart. antim. gr. i., Potass. supertart. ℥ss., Aquæ lb.ij.*
Capt. cyath. vin. 3is horis.

27th. Going on well; slight headach; skin cool; pulse 60; wound beginning to suppurate. *Mist. purg.*

28th. Continues well.

29th. Slept ill last night; complains of considerable pain and uneasiness in the head, particularly about the right temple; light disagreeable; has had no alvine evacuation for two days;

pulse 64, full, and throbbing. Venesection to ten ounces, by which the pulse was much reduced in strength and fulness. Head to be shaved, and cold applications continued. *Mist. purg.*

30*th.* Head better since bleeding yesterday. Was disturbed by the effect of the medicine in the beginning of the night, but slept tranquilly towards morning; wound suppurating kindly; pulse 60; blood drawn yesterday appears buffed and cupped.

Dec. 1*st.* Head has been painful since midnight; light, but not sound, distressing; he appears irritable in temper. Had a motion this morning; no thirst. Pulse 60, but full and throbbing; wound suppurating well. Venesection to twelve ounces. After five or six ounces had flowed from the vein, the pulse became softer, and nearly twenty beats in the minute quicker. Cold lotion to the head.

2*d.* Slept during the night, but was irritable, and once insisted upon getting out of bed to make water; bowels were not opened since yesterday morning; complains of headach; pulse 64, full. Bleeding was attempted, but owing to the smallness of the veins and the number of incisions that had been made in them, not more than three or four ounces could be obtained.

℞. *Tart. antim. gr. ij. Potassæ Supertart. ʒiij. Aq. lb. i. Capt. cyath. vin. 2dis horis usque ad nausea. Mist. purg. Cold lotion to head.*

3*d.* Headach rather increased; spoke incoherently during the night; is irritable, and refuses to allow the cold cloths to be applied to his head; wound granulating well. *Contin. mist. purg. et salin.*

4*th.* Reported much as yesterday; had two motions. He requested the cold applications to be resumed; pulse 60. *Contin. mist. salin.*

5*th.* Complains of diffuse pain of head, which is relieved by the application of cold; is irritable, but not at all delirious; pulse 60, soft and natural; tongue clean. The wound has rather a protruded appearance; its edges inclined to evert; a dark looking substance is seen arising from its centre; discharge healthy and abundant. *Contin. mist. salin. et habt. pil. ex cal. gr. i. pulv. antim. et pulv. Dover. aa gr. ss. hora quaque sexta.*

7*th.* Headach considerably abated; bowels free. *Capt. pil. hora quaque quarta.*

8*th.* A piece of bone came from the wound. The black substance has entirely disappeared. Yawns frequently, but has no headach, intolerance of light, or irritability. Pills stopped, his mouth being slightly affected. The piece of bone is about half the size of a shilling, and seems to have formed part of the internal wall of the sinus.

9th. Continues well; no headach; wound closing.

11th. Goes on in the same way. *Habeat mist. purg. pro re natâ.*

14th. Wound filled with red and rather protuberant granulations. Dressed with dry lint. From this period the wound went on healing without the occurrence of any unfavourable symptom.

He is now (July 1829) in perfect health, attending to his ordinary business, and living in his usual manner, without ever suffering the slightest pain or uneasy sensation traceable to the injury. A considerable depression, in the bottom of which is a small T shaped cicatrix, remains in the forehead, directly above the root of the nose, marking the spot first struck by the extremity of the gun breech. This extends nearly equally on both sides of the central line, and is about ten lines in breadth. Above, and to the right of the cicatrix, the bone is deficient for an irregular space of more than an inch in diameter. In this spot the pulsations of the brain can be seen from a distance of several feet. By gentle pressure on the integuments, the fingers can be sunk into the aperture of the bone, but without producing any unpleasant effect.

Merely in a surgical point of view, it is hoped that the foregoing case contains sufficient interest to excuse its being offered to the profession. The extent and nature of the original injury, and of the injudicious attempts at removing the foreign body, may well teach us how uncertain are our data for prognosis in lesions of the brain, and how much this organ can bear under peculiar circumstances. The effects also of cold applications of the various modes of depletion, and of mercury as an antiphlogistic agent, were obvious and well marked, as well as the advantages resulting from a free opening for the escape of matter. The mild character of the inflammation, and the very decided impression made by each bleeding (of ten or twelve ounces) on the circulation of a vigorous young countryman, are worthy of notice, as pointing out the anti-inflammatory diathesis which prevails to a remarkable extent among the peasantry of this part of the country.

As a fact bearing upon the science of phrenology, the case of M'Elwee seems to involve some important considerations. It is quite certain that a quantity of the substance of the brain was destroyed, and it is difficult to conceive that corresponding parts of both hemispheres did not share in the loss. Upon considering the site and depth of the wound, and the breadth of the wounding body, even granting that the latter took an oblique course, it seems scarcely possible that either of the cerebral lobes could have escaped unhurt. The principal depression occupies the situation of the organs of upper and lower individuality, and on the left side extends to that of size. On the right side the bone is deficient, and there is a slighter depression over the organ of locality. After close and frequent observations upon the man himself, however, and examination of his family and associates, I have not been able to discover the slightest alteration in mental power. He pursues his usual avocations as formerly, attending to his farm, and occasionally weaving, and exhibits no appreciable deficiency in memory or acuteness of perception. Since his recovery he has been attending an evening school, and asserts that he finds no change whatever in his capacity for acquiring knowledge.

I have endeavoured so far to give an accurate statement of facts. But, possessing a very limited knowledge of phrenology, shall not attempt any remarks upon them. Should they be deemed interesting by others better acquainted with the subject, it will give me much pleasure to afford any information that may be desired.

Letterkenny.

Art. XI.—*Sequel to a Case in which the operation for Artificial Anus was successfully performed.* By Richard Martland, M. D. Physician to the Blackburn General Dispensary.

In p. 271 of Vol. xxiv. of this Journal, was published the case of Henry Baron, on whom I operated for artificial anus in July 1824; and at the close of that communication I engaged, should any material change take place, to make it known to the profession. This engagement I am enabled to fulfil by the kindness of Mr W. Tinker, surgeon at Hyde, near Manchester, from whom I have received the following particulars.

" About twelve months before Mr Baron's decease, which took place on the 24th of March 1829, he was suddenly seized with hemorrhage from the natural anus, which brought on syncope, and he had afterwards frequent similar but less severe discharges. The bougie formerly used gave him so much pain that he discontinued the use of it; and he found most relief from the injection of a warm infusion of chamomile, part of which when injected came out at the artificial anus. His health began to decline about two months before his death, his spirits drooped, he lost his appetite, had sallowness of the skin, griping pains, and general uneasiness of the bowels, accompanied with pain in the loins, and frequent bearing down, when a brick dust coloured fluid of offensive smell escaped by the natural anus.

"On the 10th of March he began to have acute pain in the right iliac region, with swelling of the abdomen and tenderness on pressure, bowels obstructed, tongue furred, sickness, occasional hiccuping, and much fever. The case was now considered as enteritis, and treated accordingly. The blood drawn had a buffy appearance. Nothing, however, succeeded in giving relief to the pain, or in procuring a discharge from the bowels. There were constant borborygmi, which Mr Baron attributed, and perhaps correctly, to a retrograde action of the intestines. He died on the 24th, fourteen days from the commencement of the acute symptoms. An examination of the body took place three days after his death, when the following appearances were observed.

The abdomen was much enlarged; the artificial anus presented the appearance of an eschar in the left iliac region; there was no diseased appearance of the peritonæum; no fluid nor adhesions amongst the intestines. The *ileum* was dilated to an enormous size, four or five convolutions of which appeared to occupy the whole abdomen, and concealed every other viscus. Its distended state rendered it so tender, that the slightest touch produced laceration, and the escape of a quantity of fluid fæces. Five inches from its termination in the cœcum it was so contracted as barely to admit the little finger. The mucous coat was much injected for a few inches above and below this strictured part. The cœcum with its appendix and the ascending portion of the colon were filled with solid fæces of a clay colour, the transverse and descending portions empty. The sigmoid flexure was much diminished in size, and its internal surface covered with offensive mucus. The rectum in the hollow of the sacrum was ulcerated through all its coats; the ulcerated edges were ragged and thickened. The cellular substance, the ligaments and bony structure of the sacrum on which the diseased parts had rested, partook of the ulcerated state. The bone was black, and excavated so as to admit the last joint of a man's thumb.

"No contraction or inflammation was observable in any other part of the alimentary canal; no disease in any of the other abdominal viscera. The other great cavities were not examined."

From the last published account of this case to the time Mr Tinker first mentions it, Mr Baron occasionally wrote to, or called on me, and he always represented himself as far more comfortable than any one could conceive it possible to be in his situation.

From the morbid appearance in and about the rectum it is probable there was originally scirrhus, and that the canal was subsequently opened by the ulcerative process, which made such ravages in the contiguous parts; but the fatal termination of the case is not immediately attributable to the disease of this gut, but clearly to the irritable stricture of the ileum; though, had there been no other disease, the cancerous state of the rectum and adjacent parts would have certainly caused death in a short time.

It appears that by this operation nearly five years were added to this man's life, four of which were passed in comparative comfort; and I am inclined to think, that, in many recorded cases of obstructed intestines which have terminated fatally, the lives of some individuals might have been prolonged had recourse been had to it.

This operation has been successfully performed for imperforate anus by M. Duret of Brest on a child fifty-eight hours after birth. It was twenty-five months old when the case was communicated to Sebatier, who mentions it in his Medecine Operatoire, T. iii. p. 336. It has also been successfully performed in consequence of a scirrho-contracted rectum by Mr Pring of Bath. The patient was a female sixty-four years of age. She survived the operation a year and nearly four months, and then fell a victim to a cancerous state of the gut. See London Med. and Phys. Journal for January 1821 and February 1822.

In two cases recorded in Mr Pring's communication, as operated on by Mr Freer of Birmingham, the operation has not been attended with success. The first was on a child without an anus. It survived three weeks, and then died apparently from marasmus. The other was on a farmer, forty-seven years of age, who survived the operation only nine days.

In an article in the Dictionnaire de Medecine and de Chirrugie Pratiques, Mr Baron's case is particularly alluded to by that eminent surgeon, Baron Dupuytren, who concludes his account of it in the following words, "Cet exemple pourrait être imité dans les cas semblables."

Art. XII.—*On the Administration of Wine in Low Fever.*

By W. Law. Esq. F. R. C. S. E.

Though this subject belongs more to the department of the physician than to surgery, there occurred here during a late prevalance of typhoid fevers among certain classes, and those the better, so many cases of unfavourable termination, that I would willingly see the attention of the profession more fully called to this object.

Analogy, while it is ever a guide of the first importance, leaving us in no want of a rule on more trying occasions, as well as those of more usual occurrence, where the circumstances are of a nature to admit of comparison sufficiently complete, still must frequently fail in affording those results on which our confidence ought to be implicit. Food, as well as stimuli acting more immediately on the nervous system, will, in certain circumstances of health, and even in those not strictly so, confer a degree of strength, which is not necessarily transient, if the intended means have not been augmented beyond bounds; but this common, and not to be disputed, opinion, is not to be acted upon without attentive observation of many accompanying circumstances.

It is not my intention to enter here into what may be the difference between the immediate and remoter effects of very generous solid nourishment on the animal economy, or the greater or less amenability to common inflammatory diathesis, from the habitual use of strong drinks, but I wish to direct inquiry to the benefit we may expect from these last, in that state of the frame which constitutes fever of a typhoid character. It will hardly be doubted, that, even in health, food and stimuli too often or unseasonably employed, and without a certain reference to the hours of sleep, will not add to the vigour of the body; and in fever it becomes a question of considerable nicety, to determine to what extent the diseased change in the functions modifies the effects of strong stimuli introduced into the alimentary canal.

Much food, and that containing animal matter even in very minute quantity, being now so generally understood as requiring to be withheld in fever, and the patient's inclinations also generally coinciding with the denial, we need not for an instant dwell on that; but the probable and real benefit of stimuli deserves to be farther examined. While, as I have said, they have certain known effects in health, and while these are also seen to be capable of being produced to a certain extent in disease, it ought not to be assumed that there is not a bound to this, which must be very cautiously approached. We may discover, that when we fail in reaching it by the employment of the means in a certain quantity, apportioned to the individual, increase of dose, too frequent interruptions to sleep, and too short intervals between each administration, shall sometimes more than disappoint us, causing nausea, and that sinking of the vital powers which may in that manner be produced.

It will hardly be doubted, that, when we do reach the point we deśire, as indicated by the pulse at the time, slight inadvertent excess in the administration may be much to be regretted,—that there is a reaction not exactly uniform in its period of accession,—that it is sometimes sudden, and becomes difficult to calculate on. The absence of other stimuli used in infusion, to which many are now habituated, twice daily, and which are sometimes lost sight of on occasions of dangerous fever, may have depressing effects, of no slight consequence, if we are to judge in any degree from the effect on the system, when these are withheld from it in its ordinary state.

Valuable as wine, cautiously administered, and *when* indicated, is, in low fever, and fatal as typhus too often is, when treated perhaps in the best manner, yet probable injury accruing from the causes described, where there has been every attention on the part of the practitioner for the recovery of his patient, deserves careful inquiry, more particularly when we reflect that the disease is of no unfrequent occurrence, both idiopathically in the province of the physician, and as a symptom, though more free of infectious character in that of the surgeon.

So far as my own observation extends, I am of opinion, that, if strong stimuli used in a certain quantity do not produce the effect we desire, a much larger really depresses. Many, doubtless, entertain a similar opinion ; but, if correct, it is sometimes not acted upon. Knowing, as we do, that cases have recovered where much wine had been employed, it is not easy to assign the exact quantity which may be safely used ; but in general it appears that an English pint of Portuguese or Spanish wine, or an equivalent in strength of ardent spirits among water, during the twenty-four hours, ought fully to serve the purpose of sustaining the strength of adult patients in typhus ; and a much smaller quantity will on many occasions be sufficient.

The employment of mercury to the extent of free ptyalism, in very aggravated forms of typhus, is not without its advocates.

Art. XIII.—*On the Efficacy of Water-Dressing, or the Utility of Water as an application in the treatment of Wounds, Ulcers, Diseases of the Skin, &c.* By John M'Fadzen, M.D. Surgeon, Buttevant.

Having heard about two years ago of the extraordinary sanative effects of water-dressing in the cure of incised, lacerated, contused, and gun-shot wounds, as also in punctures received in dissection, in the cure of ulcers, and some diseases of the skin, I felt anxious to learn not only its mode of application, but also the theory of its action. I consider myself fortunate in meeting, about three months ago, two intelligent medical students, who had lately been pupils of that indefatigable and scientific professor, Dr Macartney of the University of Dublin, who has, they inform me, taught this method of treating wounds, &c. to his surgical class for some years past, having previously submitted it to a fair trial, although I am not aware, that either he, any of his pupils, or any other medical man, has published the result.

The medical superintendence of a dispensary, where 5000 patients on an average annually apply for relief, afforded the opportunity of trying the effects of cold water in the diseases alluded to; and accordingly, in the beginning of the month of May last, I commenced on a number of suitable cases, and have continued the same practice since that period in all cases of the kind that presented themselves, and, I am delighted to say, with a success that has considerably exceeded my most sanguine expectations; and I have little doubt that future trials of this remedy will prove, that Dr Macartney has effected, by scientific knowledge, and acute observation, a valuable improvement in the art of practical surgery.

With all deference to the learned professor, I have taken the liberty of laying before the surgical world the principle on which it is supposed to effect a cure, hoping sincerely, if any error shall be discovered in the theory, it may be publicly submitted to scrutiny. I have also taken the liberty of adding the result of my own experience of this remedy, not only for the sake of science, but on the behalf of humanity. It is true that I have only proved the utility of the theory and practice of another. However humble this office may be, I shall consider myself amply rewarded for any little trouble I have taken on this occasion, provided I succeed in directing to it the attention of the profession in general, and particularly hospital surgeons, whose opportunities of witnessing the effects of remedies of this nature in disease, enable them to ascertain their real merits. I may add that the hope, that even in a short time we shall publicly have the sense of some of my brother labourers in the surgical profession on this at present novel mode of practice, is the principal object of this paper.

Dr Macartney, I have learned, does not claim water-dressing as altogether an invention of his own, having only revived an ancient practice, to which he has added the theory or mode of cure. It was used by Patroclus at the siege of Troy, by the priests in the temple of Esculapius, by Celsus and Ambrose Paré. The latter a man of original mind, and a real improver of his art, was evidently well acquainted with its value in wounds, and wrote a treatise on gun-shot wounds, recommending a lenient method, instead of the irritating applications previously in use. He was not entirely free from superstition, however; for having used it for some time with such astonishing effects, he actually supposed its powers to depend on some supernatural agency, and therefore gave it up, saying, it was unlawful for him to perform miracles. We find La Mourier curing the Duke of Orleans' hand by means of water after it had been condemned to amputation by many other surgeons. Baron Larry used scarcely any thing else in Egypt, and so successfully, that the French soldiers ascribed miraculous powers to the water of the Nile. We also find it recommended by Mr Samuel Cooper in the treatment of gun-shot wounds in his Surgical Dictionary; and the no less celebrated Mr Guthrie, in considering the effects of poultices and cold applications upon gun-shot wounds, expresses decidedly his confidence in the use of cold water.

Dr Macartney, whilst performing some experiments on the lower classes of animals, seeing that their wounds healed without inflammation, was led to believe that inflammation was not necessary to the healing of wounds. This suggested a method of treatment by reducing inflammation as far as possible, which is most effectually done by the professor's mode of water-dressing. Wounds may heal in four different ways, *1st*, Through the medium of blood without inflammation; *2dly*, By lymph with or without the presence of inflammation; *3dly*, By granulations and pus, when inflammation must be present in some degree; and, *lastly*, By what may be styled the contracting process, as occurs from water-dressing, and where inflammation cannot exist. By this simple and novel method, not hitherto described in books, incised and lacerated wounds heal without any connecting medium, without pain, lymph, granulations, or suppuration; but, as I have just stated, they heal by a species of contracting process, from the bottom upwards, the edges at the same time approaching, leaving scarcely any cicatrix, and which is never endowed with morbid sensibility, being neither hard, gristly, or painful, like that resulting from the usual mode of treatment. Of this fact, I have had ample proof in my own practice.

The principle is to excite an agreeable sensation in the part affected; for if this be present inflammation cannot exist. The parts of which the human body is composed being naturally humid, the application of a fluid such as water, which is of the least irritating nature, affords an agreeable sensation, and prevents the necessity of inflammation, by doing away with the sense of injury sustained by the parts, it being a law in the animal economy that inflammation will not arise after an injury done to a part, unless that part feel sensible of the injury.

The mode of applying this remedy is exceedingly simple, and fortunately attended with very little trouble. A piece of lint dipped in cold water is to be applied with the soft side to the part, and covered with oiled silk, which should extend considerably beyond the limits of the lint, and retained in its place by a light bandage, or any other means the practitioner may deem proper. Any other substance capable of preventing evaporation, and sufficiently light and pliable, such as very thin Indian rubber, would answer the purpose as well as oiled silk. The dressings should be removed three times a-day, or less frequently if the secretions from the part are trifling, for the purpose of wetting the lint as it becomes dry, and freeing it from the secretions of the wound or skin, which would in a short time become irritant; therefore, it is not sufficient that the lint should be merely moist, for this moisture may be occasioned by perspiration or other discharge of the part collected under an impervious substance. Hence the lint must either be occasionally removed, or well washed in cold water, and in like manner the oiled silk or Indian rubber.

From what has been stated, it must appear that the good effects of this treatment depend on the production of steam at the temperature of the surface of the body, which, being retained by the impervious silk, subjects the part constantly to an atmosphere of that vapour.

I hope it will not be considered irrelevant to mention here, that oiled silk is also a valuable substance for applying the emollient poultice, having this advantage over linen or calico, that it retains its moisture and heat, at least the heat of the surface over which it is placed, for a greater length of time. I subjoin a case to illustrate its use in this way, and to which I not only attribute the favourable termination of the case, but even the absorption of pus after fluctuation became evident.

Oiled silk may also be used in the cure of *tinea capitis*, with a lotion of the acetate of lead, which is to be applied in the same manner as directed for the application of water, the scalp having been previously shaved, and the silk applied in the form of a well-fitted cap. Dr Macartney says it will invariably cure the eruption. In short, the only thing to be feared is its being healed too suddenly. The solution of the acetate of lead in this disease has the effect, no doubt, of subduing morbid sensibility, independent of its action as an astringent. We all know that this disease consists in a bare state of the villous surface of the skin throwing out a glutinous exudation, which incrusting the part, becomes irritating, and a sort of dirty pus is secreted under the scabs, which keeps up this irritation, and the part becomes a favourite food and habitation for vermin. I also add a case in point which resisted all other means.

In conclusion, I entreat the profession in general to give this new, but simple, convenient, and efficacious mode of cure a fair trial, and I venture to assert, that they will find it more effectual than the common method, by unctuous and irritating applications. If I succeed in turning the attention of even a few of my brethren to this subject, it will most probably get into better hands, when the object of my paper will be fully attained.

I have this moment received a letter from my friend, Mr Eames, the medical superintendant to the Castle Martyr Dispensary, who speaks of water-dressing as follows:—" I am delighted to find that you are a *water-dresser*, or, more properly speaking, a *steamer*, for in all cases it is in a state of vapour when applied to any part of the body. I am constantly in the habit of applying water in a great variety of cases, such as lacerated wounds, ulcerated surfaces, boils, cases of anthrax—in which its operation is wonderful,—whitlows, and some erysipelatous surfaces. In all the cases in which I used water or steam, the patients were surprised, and could not be persuaded that a *charm* had not been used. In cases of wounds where I use it, they heal without pain, and almost in all cases without any suppuration. You may make your own oiled silk much cheaper than you can purchase it, and quite as good, merely by nailing a piece of white silk or sarsenet (the latter I use) on a frame, and brushing it lightly over with plain boiled oil, such as painter's use." I am in the habit, in my dispensary, of making my oiled silk of old silk gowns of various colours, with which the ladies in the neighbourhood are charitable enough to supply the establishment, which I find to answer uncommonly well.

CASE I.—*Incised wound.*—Jane D., ætat. 16, a poor girl, applied to me on the 25th June last for relief, when I found the middle finger of the left hand firmly contracted on the palm, which rendered that hand comparatively useless. On proposing an operation as the only chance of remedying the deformity, she readily consented, and it was accordingly performed in the following manner. A longitudinal incision was made down on the tendon over the first phalanx, which divided the integuments and fibrous sheath which encompasses the tendon, which by a transverse incision was also divided. The finger was then straightened by some force, and secured by a splint fastened to the back of the hand. Lint wet with cold water was applied to the wound, and covered with oiled silk to prevent evaporation. She was ordered to keep the hand quiet in a sling, to change the dressings twice a-day, for the purpose of wetting and freeing it from the secretions of the part. It healed rapidly, without inflammation, suppuration, or granulation, and, unless by minute inspection, no cicatrix is observed. She now possesses the use of the finger, the tendon having re-united, while the finger remains straight.

CASE II.—*Contused Wound.*—August 14, 1829.—M. B. ætat. 10, received a very severe bruise on the great toe of the left foot, from a large stone falling on it, which shattered the nail and divided the integuments to a considerable extent. The parts were brought together and secured by sticking plaster, and water-dressing applied over it. She had little or no pain till morning, when the dressings were removed. 15th August. Looks well; little or no inflammation. Water-dressing continued. In a few days the dead parts began to separate from the living, and a bread and water poultice substituted for the water-dressing. 26th. Dead parts completely separated from the sound, including the nail; granulating process has commenced. Ordered to resume the water-dressing. It continued contracting and healing till the 18th September, when the nail had considerably advanced in its growth.

CASE III.—Showing the advantage of applying the common poultice on oiled silk.

Mr J. D. B. complains of pain and swelling of one of the lower rows of glands of the groin, which it is supposed resulted from a small abscess in the sole of the foot. Has used evaporating lotions for some time without benefit, assisted by the application of several leeches, together with the exhibition of an occasional purgative, with absolute rest and low diet. Seeing no other hope of checking its progress, I ordered a poultice of bread and water to be applied on oiled silk, to prevent the evaporation of its moisture and its consequent hardness. By this application the pain was greatly relieved, and its progress to suppuration arrested. At this time he was persuaded by some person that a poultice of some irritating herbs would reduce it at once. He applied it, but fortunately was obliged to remove it in a short time, in consequence of the pain and inflammation which it produced, and he again had recourse to the poultice and oiled silk, the only application which gave him any relief. Seeing now some chance of discussing the tumour, evaporating lotions were again tried, but did not answer, for fluctuation at last became evident, and he was now obliged to recur to his old remedy, the poultice. It was again discussed; the matter was absorbed, leaving a little hardness, which readily yielded to a solution of the muriate of ammonia.

CASE IV.—Solution of acetate of lead, in *Tinea Capitis.*—11th July, Master R. R., ætat. 11, of a flabby habit, and subject to a cough, has been labouring for some years under *tinea capitis*, or the *porrigo scutulata* of Dr Bateman, which has resisted all the common remedies, both local and constitutioual, not only under my own care, but by the advice of others. The head having been shaved and well washed with soap and water, which exposed a number of inflamed irritable patches, covering about half of the hairy scalp, I dressed the head according to Professor Macartney's plan, namely, covering the diseased scalp with charpie dipped in a solution of the acetate of lead, (*gr. iij. ad ℥i. Aq.*,) and applying over all a well-fitted oiled silk cap, leaving directions to wash the lint three times a-day in cold water, in order to free it from the secretions of the skin, reapplying it to the head, wetted with the medicated lotion. He has called upon me this day, (July 26), when I was happy to find him nearly cured, the inflamed and irritable patches having been replaced by a comparatively healthy skin.

Allow me to observe that no constitutional treatment whatever was resorted to.

I at first proposed to have added a case of ulcer. I find, however, that the case intended for that purpose, although much improved, has not yet made sufficient progress in the healing process to admit of insertion. I have, notwithstanding, speedily cured several cases of superficial ulceration by water-dressing, but of which I have taken no notes.

Buttevant, County of Cork,
27th July 1829.

I beg leave to add, that, since the date of this paper, I have continued water-dressing in all suitable cases that have been presented to me, and these have by no means been few in number; and I am delighted in being able to say, that the more I see of it the more convinced am I of its utility, particularly in incised wounds, extensive lacerated wounds, especially of the scalp, and in superficial ulceration, as is shown by the cases annexed to this paper. This dressing has a peculiar tendency to fill up chasms, formed by solutions of continuity of soft parts, as after the separation of sloughs in lacerated wounds, &c. I have likewise employed this dressing in conjunctival ophthalmia, and sloughing of the cornea, with good effect. In anomalous diseases of the skin with an inflammatory base, I have found it to be highly efficacious. Not being a military medical man, I have not as yet had an opportunity of trying this remedy in cases of punishment. I am convinced, however, that no other remedy would answer so well. Some intelligent medical officers, indeed, who have been quartered here from time to time, have promised to give it a fair trial in these cases, and inform me of the result. I have for some time back been making a series of observations regarding the comparative value of water-dressing and the common poultice as therapeutic agents in effecting the separation of sloughs; but my experience in these cases has as yet been too limited to enable me to decide exactly in favour of either, although, as far as celerity is concerned, I dare say the common poultice will be found superior in old indolent ulcers, where a stimulus is necessary to produce a healthy action in them. I am now in the habit of immersing the affected limb for some hours together in an atmosphere of steam produced by a steam-boiler invented by Professor Macartney for that purpose, and on which I conceive I shall be able to make some improvements. As I intend, however, to make the cases now mentioned the subject of another paper, I shall postpone the description of the Professor's steam apparatus, together with the principle of its action, until I be able to accompany it with its effects in those cases.

Reparation of the skin and Cellular membrane of the Scrotum after spontaneous gangrene.

CASE V.—John Finn, a labourer, ætat. 40, of good constitution and temperate habits, was, while at work in a field, on the 22d of August 1828, suddenly attacked with violent pain in both testicles, for which he could assign no cause. There was no redness or tumefaction. On the 23d, I was sent for, and I found the scrotum very much swelled, red, and exceedingly painful to the touch. I ordered him a lotion of the *Liq. Plumb. Subacet. Co.*, and, as he lived at some distance from me, desired he would let me know the next morning how he did. Having heard nothing of him for some days, I concluded that the case terminated favourably. On the 29th, when I was again called, I found the integuments of the scrotum and about an inch of those on the dorsum of the penis, in a complete state of slough, and emitting almost intolerable fœtor. His pulse was languid; his spirits very much depressed; and his countenance expressing great anxiety. I ordered him bark and sulphuric acid, and directed a fermenting poultice to be applied to the parts. 30th. The patient feels better; fœtor much less; pulse improved since yesterday. *Repetatur cataplasm. et continuentur medicament.* 31st. The whole slough came away, leaving the testicles completely exposed, as well as part of the dorsum of the penis. I now dressed the parts with two or three folds of lint soaked in cold water, and covered it completely with oiled silk, to prevent evaporation. This dressing was applied daily. As his strength was returning, I discontinued the bark. The contracting process went on gradually and progressively from the first, until the testicles and penis were completely covered with integuments. During the entire time there was no pain, no suppuration, and the granulations were so minute as to be scarcely perceptible. In six weeks he was able to resume his occupation as a labourer.

Lacerated Wound.

Case VI.— —— Cunningham, ætat 60, very stout limbed, and of a good constitution, received, on the 2d of August 1829, a wound on the right leg, from a large fragment of a rock which fell on him while he was at work in a quarry. The gastrocnemii muscles were almost entirely torn off, and five or six inches of the front of the tibia exposed. The muscles were so much contused and lacerated, that I found great difficulty in replacing them. I covered the wound with wet lint, put the oiled silk over it, and applied a calico roller with a moderate degree of pressure over all. The wound was daily dressed in this way, and this day, the 26th, I had the satisfaction to see it completely healed. I must observe that in this case there was no pain felt, and there was scarcely any suppuration. The functions of the muscles are unimpaired.

Carbuncle.

Case VII.— —— Scannel, ætat. 70, of debilitated constitution, came to me on the 6th of August 1829 with a very large carbuncle. I opened it, applied water in the usual way. In three or four days the sloughs came away, and the part was completely healed on the 25th. These three cases have been obligingly sent to me by my friend Mr Eames, medical superintendant of the Castlemartyr Dispensary in this county.

Contused wound of the Scalp with erysipelas.

Case VIII.—September 14th, 1829. M. S. æt. 24, early this morning received a very severe blow of a stick on the head, which knocked her down, and rendered her senseless for some time. She was visited in the evening, when a contused wound about three inches in length was discovered over the left parietal bone; its edges were swollen and retracted, and an erysipelatous blush was extending over that side of the head and face; tongue furred; pulse 90; slight stupor. Bled to sixteen ounces; ordered a powder of four grains of calomel and three of antimonial powder, to be taken at bedtime, and in the morning to be followed by one ounce of Epsom salts. The whole side of the head was covered with wet lint, and oiled silk applied over it, with directions to change the dressings three times a-day.—September 15th. Edges of the wound approaching; erysipelatous blush disappearing; tongue improved; pulse 76; stupor completely gone. Continue.—November 16th. Wound looks healthy, with slight discharge; erysipelas gone. From this date the wound continued gradually contracting till it healed.

Penetrating wound of the knee-joint.

Case IX.—September 16th, 1829.—P. M. ætat. 28, of good constitution, applied for advice on account of an incised wound which he received transversely on the right side of the patella of the left leg. A probe could be introduced into the joint, and the synovia escaped. The edges of the wound were brought together by a suture and adhesive plaster, expecting it would heal by the first intention. Ordered to keep the limb perfectly quiet.—September

17th. The wound has not united; synovia still escaping; little constitutional disturbance. Ordered some opening medicine, and water-dressing to the wound.—September 18th. The wound is so far contracted and filled up as to prevent the escape of synovia. Continue the dressing. No synovia afterwards escaped, and the wound healed rapidly.

Buttevant, 10*th November* 1829.

Art. XIV.—*Observations and Experiments upon the Kusia or Koosia of the Indians,—the Bitter Cucumber, Momordica operculata of Linnæus.* By John Hancock, M.D.

THIS plant, the fruit of which forms, perhaps, the most active hydragogue purgative in nature, is indigenous upon the sandy shores of the Essequebo and in various parts of the interior of Guiana; yet it had never been known as a remedy, nor at all noticed in the colony, so far as I am aware, till the year 1821, when, from its extraordinary bitterness and analogy to momordica, I was induced to make trial of it medicinally. Since that period, I have employed it with the most satisfactory results, more especially in general dropsy, leucophlegmasia, mal d'estomac, cachexy, and weakened sluggish state of the organs of digestion.

The plant is a scandent vine, having altogether the *habit* and *facies* of the *Cucumis sativa* or common edible cucumber. In the Linnæan system, it belongs to the *Monoecia Monadelphia*, natural order *Cucurbitaceæ*.

The root is fibrous. The stem is five-angled, five-channeled, climbing to the summit of the highest trees, or trailing extensively over the surface of the ground. The leaves are distant, angular, obscurely five-lobed, roughly pubescent, on long channeled petioles. The tendrils at the base of the petioles are long, divided, and spiral. The flowers are yellow, the males borne in clusters upon a long common receptacle; the females solitary, elevated above the germ upon a stout columnar receptacle, which grows and becomes the lid or operculum of the capsule. The petals of the corolla, in both males and females, are about thrice the length of the calyx, obovate, spreading, obtuse. In the males, both the calyx and corolla are deeply cleft, or joined at the base only. In the female calyx, there are five acute leaflets, which are quite distinct and distant. In all these respects, its disparity with the assigned characters of *elaterium* or *momordica* will be apparent. The antheræ, as in most of the kindred genera in this natural order, are five in number, cobering, borne upon three filaments. The stile is cleft. The germ large, angular, swelling into an oval, trilocular, prickly, brown and dry *capsule*, rather than a *pepo* or *pomum*, as stated in botanical works.* Willdenow therefore says of the fruit "intus absque pulpa, siccus." The fruit, indeed, appears, in respect to its envelope, almost as much the capsule, as in *Papaver*, *Bixa*, or *Sparmannia*, and, like the former, sheds its seeds at the insertion of the pistillum. This is mentioned merely to show a similarity in respect to the external pericarp, and for no further analogy. The lid falling, discovers within a white three-celled reticulum or web-like substance, lying loose, or but slightly attached to its envelope. This web† is exceedingly bitter, and one of the most active cathartics in nature. It forms the true receptaculum of the seeds, which are black, compressed, and numerous. The albumen is pregnant with a sweet bland oil. —Willdenow says the fruit is green, "fructus viridis," &c. Like most other fruits, it has a green colour before maturity: it then rapidly turns brown and dry.

It would seem that the locality or habitat of this plant had never been known or defined; for Linnæus, and all the later botanists, have, after Commelin, referred it to America, which is a wide field for research, if a botanist wished to identify the plant, as they do not point out any particular part of that vast continent where the plant is to be found. In fact, the little that is known of it, appears to rest solely on the authority of Commelin, who has given the figure of the plant,‡ if it be the same species.

The words of Commelin are, "Mom. Americana, fructu reticulato sicco. Ex fibrosa et parva radice, sarmenta proveniunt tenuia, viridia et *rotunda*." In his figure also, it appears as a round stem.—Of the embryo, he observes, "Qui tandem excrescit oblongum, turbinatum et echinatum: hic per maturitatem fungosus et retis instar perforatus est, in quo semina continentur, oblonga, plana et nigricantia." In a section of the ripe fruit, the web or reticulum is represented as having six lobes and six corresponding cells, and is apparently sprinkled with black dots. §

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* In the whole order the fruit is a pepo, but this becomes dry and fibrous in several *when ripe*.

† I call it a web for brevity, and it seems to me the most expressive term I can find for it; the fruit being neither a cucumber, an apple, nor a gourd, but a capsule enclosing a light web or reticulum.

‡ V. Plantæ. rar. Commelini, T. 22.—Possibly, however, it may be growing in some of the botanical gardens here, as seeds of the plant were sent to Scotland, a few years since, from Essequebo.

§ The Cucurbitaceæ have all a three-celled pepo, but several have the appearance

The Kusia, however, has usually only three cells, most rarely four, but never exceeds that number.—The stem, instead of being round, has five acute angles.—Besides, our author has neither figured, or made any allusion to the operculum, which constitutes its most singular and striking character.—It appears to me, therefore, not improbable that we have two distinct species confounded under the same names of *Momordica operculata;* but this we cannot decide, being uncertain whether Commelins' description be correct.

For the plant here described, the native name Kusia or *Koosia* as a trivial one may be most convenient, the Linnæan (if it be *M. oper.?*) being too long, I had almost said too barbarous, for frequent repetition. Knowing no English name, I had called it Bitter Cucumber; but this name is appropriated to the *Cucumis agrestis* of Linnæus; besides, the *Koosia* is neither properly a pepo nor pomum, but a dry trilocular capsule, not bursting, but having a deciduous lid at the apex. In this respect it differs widely from *M. Elaterium*, which is a succulent cucumber bursting at the base, and ejecting its juice and seeds in a peculiar manner. *M. cylindrica* has also a dry capsule, and I believe several of this genus have the same, whilst other species bear juicy, spongy cucumbers, more or less like elaterium. I suspect that the modern revolutionizing botanists have in many instances parted genera on much slighter grounds of difference than are observable in the genus *momordica.*

The web or reticulum of the fruit, which usually weighs about six or eight grains, is the active part. The seeds and the outer shell or capsule are inert; yet, as somewhat of the active part may adhere to the seeds and shell, I have generally employed the whole fruit.

I have paid no attention to the chemical affinities of this substance. On cutting the green fruit, I have observed that it blackens the knife, from which I judge that it contains much gallic acid. If there be such a thing as a *bitter principle,* this assuredly presents it, for it is one of the bitterest of known substances. In this respect, the quassia bears no comparison with

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of six cells from the alternate position of septa and several receptacles. See this structure well illustrated in Dr Hamilton's paper, Trans. R. Soc. Ed. Vol. xi. part i. which see.

In respect to the fruit of the Cucurbitaceæ, authors, we find, differ much as to the structure observable in the several genera; but it certainly does differ greatly in this natural order. Authors represent those of the genus *cucurbita* to be a pepo with from three to five or six cells. *Sicyos*, monospermous; in *Bryonia*, a berry; in the genus *Elaterium a capsule unilocularis*, 2 ad 3 valvis, Decandolle; in *Erythropelum* of Blum. *unilocularis*, *trivalvis*. Cucurbitaceæ 1 *S. multilocularis*, Persoon. *trichosanthes* 1 ? 3–9 *locularis* Decandolle, Prodrom. pars. iii.

it. The web, agitated in water, gives out much froth like soap.

As to its *modus operandi*, I may observe, that, whilst it evacuates the intestines, it unloads the cellular system of serous deposit, unburdens and accelerates the circulation, has much effect in glandular and visceral obstructions, in improving digestion and rousing the alimentary functions. This no doubt is partly owing to its potent bitterness, for we find the infusion very bitter, although in the proportion only of one grain of the decorticated portion or web (which is a common dose) to a tumbler full of water.

For the first two or three years I used the koosia alone, or without any other adjunct; but, finding the infusion, which seemed in some respects preferable to the tincture, to spoil very soon, I entered upon a long series of experiments for finding a preservative which should at the same time moderate its action upon the stomach, as being liable to provoke vomiting. After trying various acids, alkalies, oils, sugar, neutral salts, &c. I could find no advantage was gained, excepting in the combination with common salt or cream of tartar, by either of which adjuncts the infusion would not only keep for many months, but its operation was considerably milder on the stomach, and with more certainty directed to the bowels, so, indeed, as very materially to improve the remedy, seldom causing vomiting in the common dose (one grain,) unless the stomach be foul.

In this way, two of the capsules put into a quart or porter bottle, with a tablespoonful of common salt, half filled with boiling water, shaken several times, and afterwards filled up with boiling water to the neck, and agitated several times while cooling, will make an infusion, of which from half a wine-glass to a glassful, or from one to two ounces, may be taken for a dose, according to the patient's strength and habit of body. If the fruit should be deteriorated by age or moisture, a larger dose may be required, which in this form will be easily ascertained by trial.

It may be prepared in the same manner with the supertartrate of potash; but, as a larger portion of water is required to dissolve this salt, I have usually prepared it with four times the quantity of water, and, consequently, a quadruple dose of this was given.

The first of these preparations is the most convenient and equal in all respects to the second, as an ordinary purgative; but, in dropsical cases, the second (*i. e.* with super. of potash) may be preferable. With either, however, patients express their astonishment at the quantity of water carried off by stool, even when they have drank nothing for the whole day, or for the preceding twenty-four hours. It is not a little amusing to hear the negroes discussing this subject at times among themselves. I have heard them remark, that the water was sucked up out of all their skin,—by all the skin meaning the whole body. Notwithstanding its extreme bitterness and the nausea it will often occasion, I have observed them fond of taking it in many cases, as, when they are really ill, they esteem a medicine the more highly when it *works them off well, as they call it.* When not so, any thing will do as a placebo.

With an additional teaspoonful or two to the dose of cream of tartar in either of the foregoing preparations, it forms a most potent hydragogue cathartic, diuretic and sudorific, especially if taken warm. In smaller doses it is a very capital alterative and stomachic. Its use, whether in full or alterative doses, is usually followed by much increase of the appetite. The patient should be covered warm, and lie still in bed. It usually acts with much effect on the skin, bowels, and kidneys,—in short, on the whole system. For delicate stomachs, the addition of liquorice renders it much less offensive.

I have always found it best to give this remedy at one draught as a cathartic; for, according to my experience, it does not act well in divided doses, as directed with elaterium; and, notwithstanding its activity, there is nothing to be feared from a full dose, for, should the evacuation prove excessive or harassing to the patient, it is immediately controllable by a small dose of opium, with the use of barley-water, starch glysters, with tincture of opium if required. This I have proved from trials made on purpose, for I never had actual occasion for it. In the fluid form, in which it is exhibited, it is certain of being rejected when taken in an over-dose; and, if the extract of elaterium were or could be given in this way by infusion, it is probable that purgatives would seldom be found to operate with too great violence, as often reported of it.

When a full dose of the bitter infusion is taken, the patient should lie down and remain tranquil for an hour or two, in order to prevent vomiting, and should be provided with a flat basin to enable him to spit without raising his head and shoulders, by which means he will, in a great measure, avoid the disagreeable nausea which is apt to attend the medicine. With this precaution it will seldom cause vomiting. Although exciting, however, no sensible nausea, it will augment the flow of saliva, and indeed it appears to promote the secretion of mucus or slimy fluids in the alimentary canal along its whole course.

I have commonly repeated the dose in ascites after three or four days, according to the strength of the patient, and have also given it in universal dropsy with complete success, even in the most forlorn cases. It should be commenced early, however, to give it a fair chance of success. In the advanced stage more caution is requisite.

I must here observe, that, when it is not early perceived to produce favourable effects, as reducing dropsical swellings, &c. I have usually found a gentle mercurial course to be attended with the most decided advantage, and that, both by its own proper agency, and as a preparatory, or as reducing the system of the patient to a condition susceptible of the action of other remedies.

I am aware that mercury is often employed by practitioners in dropsy, but upon too limited and narrow views, being restricted, by a capricious dogma, to those cases only which appear to depend on a diseased state of the liver. Many who have observed the efficacy of mercury in dropsy,—who have a servile deference for fashionable though often absurd maxims of pathologists,—and who shape their practice upon what they conceive established and unerring principles, may be observed racking their brains to make out some affection of the liver, so as to get a plea for prescribing mercury,—the oracles having taught them that mercury is to be resorted to in dropsy, only when the functions of the liver is disordered; and thus is the healing art fettered, in thousands of instances, by the dogmas of physic.

According to my experience, there is no tribe of diseases in which mercury, under proper management, is more generally advantageous than in dropsies; but it appears to me to be equally so whether the liver be diseased or not. In some cases I have found it necessary to repeat salivation, and even to employ paracentesis withal.

In a most obstinate case of ascites and general dropsy, which occurred about two years since in a black woman, (Frankey,) on plantation Better Success, Essequebo, I found it requisite to draw off the water by tapping eight or nine different times, and to salivate her thrice. When I left the coast, she had been several months at her work, hale and strong, without the smallest symptom indicative of a relapse. In this case the kusia held only a subordinate place in the cure; and this case is cited rather as an instance of the failure of the remedy singly employed, and to indicate the additional means which were resorted to with complete success. Decoctions of the bark of *Amyris Juribali* and ginger were also given to restore the strength during and somewhat prior to convalescence. The kusia alone, however, has often effected cures in the most forlorn cases of general dropsy.

Its effects by glysters, in procuring operation in dry belly-ach, exceed any thing I have yet tried. It causes some sickness at the stomach at times, but less than when taken by the mouth; yet it appears to empty the whole canal, attended, perhaps, with a less secretion of water.

I have also employed it by enema with happy results, in two cases of enlargement of the spleen, one of which was in my own person about three years since. This swelling arose during the progress of an ardent fever, which was succeeded by an intermittent; and both the fever and painful swelling were removed by four or five enemas of kusia.

To prevent its too ready action on the lower intestines, the injection should be very dilute, as one web to a quart of water, of which from four to six ounces may be injected, and repeated as may be requisite. A little soap and sweet oil is a useful addition as emollient and sheathing. In this mode, employing it cautiously, in small quantities, and at distant intervals, I am disposed to think it may prove a potent resolvent in cases of obstruction and enlargement of the spleen, perhaps of the liver and other viscera, prepared as already noticed.

In 1825, I gave some of the fruit of the kusia to Drs Robson and Allen, who effected several cures of dropsy with it in Demerara; and they several times sent for further supplies. Dr Robson, who has come home to settle in Scarborough, can give his own account of it. This is the only reference I can make to any medical testimony on the subject. Being, however, so abundant that cart loads of it could soon be collected in Essequebo in the months of March, April, and May, experiments with it may easily be instituted on a large scale, as in hospitals especially.

I may conclude with a few remarks on the officinal elaterium. I had some samples of it sent out to Demerara, which were all widely different in power, some being almost inert. This I thought arose from the strange mode directed for their preparation. The active principle in kusia is soluble both in water and spirit: it would seem not to be so in elaterium, if we may draw an inference from the mode of preparation directed by the colleges. That of the Edinburgh College seems to me quite unintelligible. * If the active part be at all soluble in aqueous menstrua, then a large portion must be lost, *i. e.* thrown away in the supernatant liquor or juice, besides what must remain in the pulp when but slightly pressed. Hence the minute portion of extract obtained and its extravagant price. †

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* This, however, it appears from an observation of Dr Thompson, is now abolished: "It is very remarkable that the Edinburgh College has rejected so important a remedy from the last edition of its Pharmacopœia."—London Dispensatory, p. 766.

† From a subsequent consideration of the subject, it appears that the active prin-

"From Dr Clutterbuck's experiments, it appears that the quantity of elaterium is so small that only six grains of it are procured from forty cucumbers. Dr Paris found that ten grains of the best elaterium, as it is found in the shops, contains only one grain of elatine; and he observes, that in general it is adulterated with starch, on which account we scarcely ever obtain two samples of it of the same strength."—L. C. p. 765.

The fructus kusia has the advantage of uniformity in point of strength and activity, if gathered about its maturity, and kept from moisture, and it admits of no adulteration. Each web containing on an average about seven grains or seven full cathartic doses, presents a great contrast with that just cited of elaterium.

It may here be observed, that the common *extractum elaterii* of the shops, so variable and uncertain in power, is in general, however, found to act in a dose of from half a grain to two grains, *i. e.* in about the same quantity as the *web* of kusia by infusion. An ounce of the extract will cost in the shops about L. 3 Sterling, according to Mr Gray's Supplement to the Pharmacopœias. The web of kusia need not cost more, I should suppose, than 4d. or 6d. the ounce, containing at least an equal number of doses. This may be considered a great advantage to the poor, although it may be said they stand more in need of food than physic.

The elaterium of the shops, indeed, from the roughness and extreme uncertainty in its operation, has by many, and perhaps with reason, been considered a medicine not unattended with danger.

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ciple in elaterium is not soluble in water, as in some pharmacopœias, (as that of Van Mons, &c.) the fecula is directed to be washed, *well washed* in water, by which it plainly appears neither to be soluble in simple water, nor its own proper juice. This insolubility in water indicates its resinous nature, which may in some measure account for its acrimony and drastic operation on the bowels; on the contrary, the active principle in kusia being soluble in water, may account for its greater mildness of operation.

PART II.

CRITICAL ANALYSIS.

Art. I.—*Precis d'Anatomie Pathologique.* Par G. Andral, Professeur à la Faculté de Museum de Paris, &c. *Outlines of Pathological Anatomy.* By G. Andral, &c.

In the second part of his work, which is devoted to special pathological anatomy, M. Andral treats successively of the morbid changes incident to individual organs and sets of organs in the following order;—Diseases of the Digestive Apparatus, of the Circulating Apparatus, of the Respiratory Apparatus, of the Apparatus of Secretion, of that of Generation, and finally of that of Innervation.

The first class of diseases is divided into two sections, according as they take place in the infra-diaphragmatic or the supra-diaphragmatic portion of the alimentary canal. The first chapter of section first contains a view of the ascertained facts regarding the structure and appearance of the alimentary tube in the state of health, in the course of which M. Andral does ample justice to the researches of MM. Trousseau and Rigot, and of M. Billard. These, as they have already occupied our attention sufficiently in this Journal, it is superfluous at present to reproduce. His account of the glands of Peyer is indistinct, and contains nothing new.

The morbid conditions of the alimentary canal the author then proceeds to consider under the following five heads,—1. lesions of circulation, including hyperemy and anemy; 2. lesions of nutrition, including the varieties of hypertrophy, atrophy, softening, ulceration, perforation, changes in capacity, and the varieties of congenital malformations; 3. lesions of secretion, comprising those of the mucous membrane and of the submucous tissue; 4. entozoa, or the several forms of intestinal worms; and, 5. states of the digestive canal in which during life its functions are disordered.

Passing over the first article, which contains nothing new, we find in the second hypertrophy considered first in the mucous and then in the submucous tissue. The former is described as that state of the gastro-intestinal mucous membrane in which it is harder and more resisting than in health, and in which extensive patches may be removed without laceration, leaving the submucous tissue untouched. It is accompanied either with the healthy tint, which is rare, with different shades of redness, with a slaty colour, or with a brown or black hue. This change is more common in the stomach and in the large intestines than in the small, and in the first organ it may be either smooth and uniform, or assume the appearance of numerous mammillary elevations separated by depressions, while in the colon it generally occurs as a uniform thickening of the mucous coat. M. Andral also notices a variety of hypertrophy of the gastric mucous membrane, in the form of irregular patches, rising from half a line to two lines above the surrounding membrane, and coloured red, brown, or gray, or even sometimes of a dead milk-white. In regarding these patches as hypertrophy, however, we must observe M. Andral loses sight of the distinction between inflammation and its effects, to which they are to be ascribed. In short, it is manifest that what British physicians regard as traces and effects of the inflammatory process, M. Andral at once pronounces to be hypertrophy, and thus multiplies distinctions without adding to the utility of his work. In inflammation, the British pathologist always recognizes a process, which, if not terminating spontaneously, or arrested by art, tends to the deposition of matter in the organic interstices, and consequently to the thickening, the condensation, and the induration of tissues. These changes, on the other hand, in the eyes of M. Andral, constitute *hypertrophy*, which is thus converted from an ulterior stage of the inflammatory process, into a separate, independent, and characteristic morbid condition.

From this criticism, however, we are willing to exempt another variety of hypertrophy, distinguished as partial by the present author, and which consists in the formation of sundry new products at the surface of mucous membranes, designated by the names of excrescences, vegetations, fungi, polypi, cancers, and which M. Andral asserts he has ascertained by dissection to be nothing but different forms of hypertrophy. Of all these productions, the general character appears to consist in a morbid excess of growth in the mucous membrane, sometimes with preternatural hardness, sometimes with unusual softness, in some instances colourless and void of vessels, in others, however, with a liberal supply either of arteries or of varicose veins, and occasionally with such a developement of capillary system as to constitute a species of erectile tissue. The shape of these new productions varies. M. Andral mentions one case in which the inner surface of the stomach was occupied by numerous plates arranged on a level, rising from two to five lines above that of the membrane, cut perpendicularly by the longitudinal axis of the stomach, and formed entirely by excessive developement of the mucous membrane, and communicating to the stomach the appearance of the *maniplies* of the ruminating animals. Of others the shape varies according to no uniform rule—being sessile or pedunculated, pointed, rounded, or that of a stalk terminating in a rounded head, not dissimilar to the *pileus* of a mushroom, or the head of cauliflower.

Though these vegetations may occur in any part of the canal, they are more frequent in the stomach than in the duodenum and ileum, and M. Andral distinguishes them in the former situation according as they occupy one of the surfaces of the organ, its cardiac orifice, or its pyloric orifice. He further represents them as becoming, next to the stomach, more frequent in the cœcum, colon, and beginning of the rectum, at the lower part of which they are met, he says, more frequently than elsewhere, and in which they may be ascribed to the operation of a specific cause, the syphilitic virus. It must be observed, nevertheless, that it is by no means an ascertained fact that the vegetations which arise from the latter cause are of the same nature, or to be placed in the same class with those which are found in other parts of the alimentary tube.

To illustrate the nature of some of these productions, M. Andral mentions an instance in which he found in the inner surface of the same stomach seven vascular vegetations, each as large as a nut, with a mammillated head, and attached by a broad basis to the mucous membrane, of which they were manifestly a simple developement or prolongation. In like manner, in a stomach presented by M. Rattier to the Royal Academy of Medicine, the surface was occupied by twenty-four small roundish tumours, each the size of nuts, of hard structure, and evidently formed at the expence of the mucous membrane, of which he tells the reader, rather unintelligibly, that they are to be regarded as fractions. Indeed, the great defect of this part of the work of M. Andral is, that he nowhere adduces any facts which can positively and satisfactorily illustrate the mechanism of the formation of these bodies; and when he describes their obvious physical characters, he thinks his duty is completed. It is possible that he thinks their origin is sufficiently accounted for by regarding them as examples of hypertrophy; and we admit, as we have already done, that they are better chosen than his first examples of this morbid deviation. But M. Andral might have rendered his description at once more interesting and more useful, if he had employed the numerous facts of which he must be in possession, to elucidate the nature and characters of the process of hypertrophic augmentation.

On the subject of hypertrophy of the villosities, nothing interesting is communicated; and that of the follicles is shortly brought under notice. This appears to be merely the second stage or the effects of inflammation, for M. Andral admits that hypertrophy is very generally preceded by hyperemy, and whether it is or is not, the disorder is attended by diarrhœa. The anatomical change which takes place in these glands is first, increased vascularity and enlargement; and secondly, closure of the orifices in consequence of the swelling of the glandular substance. In some instances, however, the orifice appears to undergo enlargement,—a fact which is explained by the circumstance of the inflammatory action affecting the basis rather than the summits of the glands, and thus as it were pushing up the sac or cavity of the individual follicle. In this case the anatomist must not confound enlargement of the orifice with ulceration, to which it bears some resemblance.

Hypertrophy of the tissues subjacent to the mucous coat affects either the submucous cellular tissue or the muscular fibres, and has been long vaguely known and mentioned under the general denomination of *scirrhus* of the stomach or of the bowels. This M. Andral regards as undoubted of the several stages or degrees of hypertrophy of the intestinal cellular tissue. Thus, in many instances of chronic diarrhœa, in which the colic mucous membrane has undergone various alterations, the subjacent cellular tissue is much more conspicuous than natural, sometimes several lines thick, hard, of a pearl white colour, without blood-vessel, composed in some instances of fibres and plates regularly arranged, in others of a homogeneous substance similar to that of imperfect cartilage. In this state it is regarded as mere induration of the submucous cellular tissue. If it become more considerable and more circumscribed, so as to form at a certain point of the canal a definite tumour, it is then pronounced to be *scirrhus*. In the latter case, however, the change is merely a more advanced stage, and more intense degree of the former. "In the whitish layer," says M. Andral, "which surrounds the colic mucous membrane, and in the bulky tumour which incloses the cardia or the pylorus, the same disposition, the same texture, the same anatomical elements may be recognized; and if the name of *scirrhus* is to be applied to the latter instance of morbid structure, it ought to be with the understanding that it designates only the advanced stage of the former. Often, indeed, in this hypertrophied tissue new structure is developed; numerous vessels are formed; the mass is divided into separate pieces or lobes; or cavities or areolæ are formed; and finally different morbid secretions are produced.

Hypertrophy is rarely confined to the submucous cellular tissue only, and frequently affects the muscular fibres, or at least the intermuscular cellular tissue, which then appears in the form of white lines or cellulo-fibrous septa, interposed between the muscular fasciculi, and by this species of isolation rendering the fleshy layer in some degree lobulated. When, instead of opaque white lines or thin plates, hard white masses, more or less considerable in size, are interspersed between the fasciculi, this denotes a more advanced and finished degree of hypertrophy of the intermuscular cellular tissue; and as these masses increasing in size progressively usurp the place of the muscular fibres, the latter are separated and stretched till at length every trace of them disappears, and nothing is recognized between the peritonæum and the mucous membrane, save a mass of cellular tissue, either indurated and hypertrophied, or occupied by different organic changes.

The change now described may take place in any part of the alimentary canal, but is most usual in the pyloric end of the stomach, more rare in the cardiac or œsophageal end, not very common in the ileum, more frequent in the ascending and descending portions of the colon than in the transverse arch, and, next to the stomach, most common in the lower end of the rectum. The effects which it produces in enlarging or diminishing the capacity of the affected bowel may be easily imagined. Hypertrophy of the submucous cellular tissue round the anus often produces circumscribed indurated tumours, which are liable, according to M. Andral, to be mistaken for hemorrhoidal tumours. He represents them as similar to those found on the skin in elephantiasis, and as traversed by large blood-vessels.

Hypertrophy of the submucous cellular tissue is not equally common at all ages. Exceedingly rare in infancy and early life, it begins to become frequent after the age of 35, at which period any irritating cause appears to operate very easily in establishing the process which terminates in hypertrophic induration. M. Andral states that the organ most usually affected at this time of life is the stomach, and if the fact were established on indisputable grounds, the explanation might probably be found in the circumstance of the constant irritation given to the tissues of that organ by habitual intemperance. After the age of 65 this disease becomes more rare, and in aged subjects, who have long suffered from dyspeptic symptoms, softening is more frequent than hypertrophy.

Another form of the disease in which the muscular coat is not extenuated and destroyed, but actually hypertrophied, is that in which a section from the mucous to the serous membrane exhibits the following order of textures. 1*st*, The mucous membrane either sound or changed in different modes; 2*d*, a milk-white layer varying in thickness from one line to several inches,—the submucous cellular tissue; 3*d*, a bluish semitransparent layer, remarkable for brilliancy, traversed by white lines, evidently the hypertrophied muscular coat; and *lastly*, a dull white layer, homogeneous in appearance, like that subjacent to the mucous membrane, and which is in truth the subserous cellular tissue thickened and indurated. In this instance hypertrophy of the muscular coat keeps pace with that of the cellular tissue; whereas in others the muscular tissue alone may be affected, while the other remains unchanged. This change is also most frequent in the pyloric portion of the stomach; and this M. Andral is inclined to ascribe to the frequent excitement and action which the pyloric muscular fibres naturally undergo, and which tends, he argues, to increase their size preternaturally.

The different changes of structure now enumerated, M. Andral represents to be the same as those which have been vaguely and generally spoken of among physicians as *scirrhus* or *cancer* of the intestinal tube, which he maintains consists, in the majority of cases, of thickening and induration of certain portions of the tube to a greater or less extent, attended with corresponding changes of secretion. In a subsequent attempt to trace them to changes in the vascular, the lymphatic, and the nervous systems of the affected organs, nothing satisfactory or positive can be recognized.

Omitting a short sketch of the characters of *atrophy*, we proceed to *softening* or *pulpy disorganization* of the alimentary canal. This is either insulated, occurring in the mucous membrane, or simultaneously affecting all the tissues.

The anatomical characters of this change, which has been studied chiefly in the stomach, vary according to the stage of the process. In the beginning the mucous membrane appears chiefly to undergo a loss of its natural consistence, to such an extent that it is reduced into pulp between the teeth of the forceps. Afterwards it is converted into a pulp, seemingly inorganic, deposited uniformly over the inner surface of the organ; at a period still more advanced, this pulp, having been detached, is entirely wanting, and the submucous cellular tissue is exposed; and still later these decoriated places are so multiplied and extended, that by coalescing, a large cellular surface is exposed with only a few scattered shreds of mucous membrane left. This may be regarded as the extreme degree of pulpy destruction of the gastric mucous membrane.

It is more common, nevertheless, to observe this change in its partial state, when it is most frequent in the splenic portion of the stomach,—a state generally connected with dilatation of the veins distributed to the cardiac region of the organ.

In other circumstances, again, it takes place in circumscribed patches, which may give rise to ulceration, and terminate in perforation or rupture. The colour of the softened portion or portions may be natural, bluish, milk-white, or red, or brown.

Softening affecting all the tunics of the gastro-intestinal tube, renders them soft, friable, and as it were liquefied, or converted even into a transparent jelly-like matter, without trace of organization, and giving way at the parts so changed on the application of the slightest force. Though this change, which has been long observed by British physicians, was first described under the name of digestion by J. Hunter, and afterwards by Mr Allan Burns, and one variety of it was made the subject of a particular memoir by Dr Gardner of this place, M. Andral appears to be in happy ignorance of the researches of these authors, and quotes only the paper of Cruveilhier, and a thesis by Camerer. The subject has given rise to a good deal of discussion as to its essential cause; and, though M. Andral also attempts to ascertain the circumstances on which its occurrence depends, he is not more successful than his predecessors. From the facts which he adduces he infers, in the *first* place, that softening of the stomach may be classed with those changes which may, in certain circumstances, take place after death; *secondly*, that during life the stomach may be in such a morbid condition, that, after the cessation of life, it may be softened under the operation of causes which in the healthy state could exercise no influence on it whatever; and, *thirdly*, that, nevertheless, softening may take place during life, in consequence of a process of irritation, acute or chronic, the existence of which, however, he admits to be conjectural, and at present incapable of demonstration, either by the presence of external signs, or the evidence of pathological anatomy.

Connected with this are the subjects of *ulceration* and *perforation* of the gastro-intestinal tunics, which come next under review. These, however, have been so repeatedly brought before the readers of this Journal in the course of examining the writings of Lewis, Abercrombie, Billard, and others, that we need not apologize for omitting them. The subject of spontaneous perforation, or that which takes place during a state of apparent health, gives rise to some inquiry as to the causes on which its occurrence depends; but, on this point, M. Andral, who admits, that, of a certain number of these cases, the cause is still unknown, is not more fortunate than his predecessors.

Nor in his subsequent inquiry into the means of distinguishing spontaneous perforation from that which is the consequence of poisoning, has he been able to arrive at any other conclusion than that anatomical inspection does not certainly furnish useful information, but merely supplies probable evidence, which must then be considered in connection with the historical and moral circumstances of the case.

On the subject of *invaginations*, which is treated in the third article of the sixth section, we find some judicious observations. Several of these malpositions of the portions of the intestinal tube he properly represents as formed at the moment of death, and remarks, that when we reflect on the singular contractions which we see the digestive tube undergo in animals, it is rather astonishing that invaginations are not more frequent than that they are so seldom seen in the inspection of the human subject. From the great extent of the mucous membrane, its loose connection with and free motion in the muscular tunic, invagination of one portion of the ileum within another is doubtless within certain limits a necessary effect of the peristaltic motion even in the state of health; and when the muscular fibres are in any way excessively irritated, it is easy to understand how invagination so complete as to draw one portion of the peritonæal coat within the other is effected. It is then that, as M. Dance remarks, the contact of the serous surfaces may, in the event of inflammation, favour adhesion, and the invaginated portion be expelled from the anus without ulceration or perforation of the peritonæum. That the ileum should be the frequent seat of invaginations, and that these, if inconsiderable, should give rise to no external signs or constitutional disorder, is not wonderful; when the great length, the free motion, and the great extent of its antimesenteric attachment are considered. But it is more difficult to understand why in some instances it seems to give rise to the most formidable symptoms of obstruction, and to induce death itself. By M. Andral, these inauspicious results are regarded as connected with the extent of the invaginated portion, more especially if attacked by inflammation terminating in gangrene, when death is the result either of the shock given by the severity of the constitutional disorder, or of secondary peritonitis in consequence of perforation.

A more favourable termination, nevertheless, has been now repeatedly witnessed,—that of the discharge of a portion of intestine after long and obstinate obstruction of the bowels, followed by re-establishment of the health. Cullen, we believe, was among the first who pointed out the possibility of a considerable portion of the intestinal tube being discharged by stool, without terminating life;* and the phenomenon was afterwards observed by Mr William Dougall,† Dr Sanden,‡ and Mr John Bower.§ Baillie also records the case of a portion of the colon voided without terminating life ||; and Mr J. M. Bowman mentions in the ninth volume of this Journal a case in which the colon, cœcum, and attached mesocolon, are reported to have been discharged. Ignorant of these examples apparently, M. Andral cites from Hevin two cases, one in which 28 inches of ileum, another in which 23 inches of colon, were expelled,—a case by Cruveilhier, in which 18 inches of an intestinal loop, with a portion of mesentery, were discharged,—and a fourth case in which MM. Bouniol and Rigal sent to the Royal Academy of Medicine a portion of ileum about 30 inches long, also with attached mesentery. The subject of the last case died three months after, in consequence of peritonitis, induced by the intemperate use of cherries; but there was no inspection,—a circumstance to be regretted, as it would have shown, M. Andral thinks, that the cicatrix of the detached portion was lacerated. This fact, indeed, is supplied by the particulars of a case from Hevin, who, twelve days after the expulsion of the whole of the cœcum, found that bowel wanting, the ileum terminating directly in the colon, and at the point of union an opening leading to a purulent collection placed before the right kidney, and bounded on every side by false membrane. In another case recorded in the *Bulletins* of the Philomathic Society, and in which death took place forty-four days after the expulsion of a portion of ileum, the two extremities of the intestine, perfectly united, appeared to have been accurately adjusted to each other; and though the point of union had contracted strong adhesions to the peritonæum, the capacity of the intestine was not sensibly diminished.

The occurrence of congenital lesions of nutrition, or malformations, which are treated in the seventh section, is regulated very much by the laws, according to which the developement of the alimentary canal is affected. It is remarkable, that, though instances of entire deficiency of the cerebro-spinal axis, and even of the heart, are not unknown, we possess none of complete want of the infra-diaphragmatic portion of the tube; and the morbid deficiency is confined to one or more of its constituent parts. Of these again the liability to preternatural deficien-

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* Essays, Physical and Literary, Vol. ii. p. 395.

† Medical Commentaries, Vol. ix. 278.

‡ Annals of Medicine, vi. 296.

§ Ibid. vii. 345.

|| Account of a Singular Disease of the Great Intestines in Transactions of a Society, Vol. ii. p. 144.

cy varies, certain parts being more frequently wanting than others. The explanation of this fact is found in the history of the developement of the canal, which commences in the way of extension or progressive prolongation, from the *vesicula umbilicalis*, which is found either out of the abdomen in the shape of a small canal, along the umbilical chord, or, if the abdominal parietes are already formed, within them, forming a tube of variable dimensions, and terminating in a *cul-de-sac* at its two extremities. It is unfortunate that the exact mode of formation, whether by gradual extension and accretion from its initial to its terminal point, or by the growth of isolated portions which eventually coalesce, is not established,—the latter being the opinion of Wolf, while Oken, Meckel, and Geoffroy St-Hilaire represent it as proceeding from the umbilical vesicle upwards towards the stomach, and downwards towards the rectum. In general, the deficiency occurs at either of the ends, and the stomach is much more frequently wanting than any other part of the canal. If an authentic case could be found in which this organ was complete while other parts were wanting, as that by Elben is stated to be, it might be admitted that the alimentary tube is formed in separate portions, which eventually coalesce. The case alluded to, however, is somewhat apocryphal; and the point must be regarded as undetermined.

These congenital alterations in the nutrition of the alimentary canal, M. Andral arranges under the four heads, of errors in configuration, in dimension, in position, and in the occlusion or closure of one or more of its natural orifices. Of the first, examples are found in the central contraction of the stomach, in its division into compartments, so as to resemble the maniplies of the ruminating animals, and in the absence of the large recess. The second is exemplified in the diminution of the number of convolutions of the ileum, which then is a straight tube passing directly from the cardia to the rectum, in the absence of the cœcum, in the unusual size of the stomach, or of the cœcum, in the duplicity of the colon, as observed by Brugnoni, or in the excessive enlargement of one part with an extreme diminution of another of the canal. Of the same description are the *diverticula*, the occurrence of which is ascribed by Meckel to the vestiges of the primordial bowel, as formed by the *vesicula umbilicalis*,—an opinion which is opposed by M. Andral, *first*, because it is not certain that the rudiments of the canal are formed at this point; and, *secondly*, because *diverticula* are found in parts of the tube to which the *vesicula* does not extend, *e. g.* the *duodenum* and *rectum*. The errors in situation we omit. Of those in which one or more of the natural openings are shut, the mechanism is easily understood, by the absence of one or more of the constituent tissues, causing adhesion or coalescence of those which are contiguous.

Lesions of secretion are classed by M. Andral under the general heads of those derived from the mucous surface, and those proceeding from the substance of the tissues, the former of which is subdivided into natural secretions, morbidly increased in quantity, and morbid or new products.

The preternatural increase of the natural secretions consists either in the excess of mucus, of perspiratory vapour, or of gas. To the second cause he ascribes the serous diarrhœa, and the disease described by some of the older authors as dropsy of the stomach, and which consists in the extraordinary quantity of serous fluid contained in this organ, with extreme distension of its parietes. The presence of the unusual proportion of gaseous fluid is ascribed to irritation of the mucous membrane, or of the Peyerian glands.

Among the second order of secretions or morbid products are placed blood, melanotic deposit, purulent matter, tubercular matter, pseudo-membranes, and calculous concretions. Of the latter he details five cases, with the view of illustrating the effects which they produce upon the economy. In other respects the remarks of M. Andral are neither new nor very interesting. Indeed, this section illustrates in a remarkable manner the faulty principles upon which the work is composed. The subjects of which it treats are the effects of certain derangements of function, and belong to the domain of pathological physiology rather than anatomy; and if they are admissible in a work on the latter subject, they should have been classed as the effects of deranged action in the respective tissues to which they belong. To the secretions of the submucous tissues, viz. blood, serum, adipose deposit, gaseous exhalation, purulent secretion, melanotic deposit, and tubercular deposit, the same remarks are applicable. Indeed, we have rarely seen a more stale, flat, and unprofitable enumeration of matters of fact without any general connecting principle.

Of the *Entozoa*, a tolerable account is given, according to the systems of Rudolphi and Bremser.

In the fifth article we turn to the second section on the *state of the alimentary canal in fevers*, with the hope of finding some interesting matter on a subject which the author has bestowed much attention. In this, however, we regret to observe that we recognize either ignorance or misrepresentation. That M. Andral should assert that Pinel was the first to establish an order of essential fevers, and to distinguish these from inflammations, is one of the most extraordinary examples of ignorance of medical history which has been exhibited. Is it possible that M. Andral has ever looked into a work first published about fifty years ago, by a physician of the name of Cullen, and which is not entirely unknown in the literature of medicine? In that work he would have found all the inflammatory diseases in which fever is merely an effect of local disorder, carefully distinguished from the idiopathic fevers, or those which M. Pinel and his followers call *essential;* he would have found a clear and rather ingenious statement of the doctrine which he also ascribes to Pinel, that ataxic fever is seated in the nervous system; and, lastly, he would have found that it was not Pinel but Cullen who deprived the humours of their part in pathology, and who showed himself to be, if not an exclusive, at least a highly rational solidist. Really M. Andral calculates rather too liberally upon the credulity and the ignorance of his readers; and we did not expect, among other errors, to have to rectify such a misstatement in the history of the science.

It is difficult to understand upon what principle this extravagant incense is offered to the memory of Pinel, who doubtless is entitled to the merit of reproducing to the physicians of France the nosology, pathology, and semeiography of Cullen, and giving them some idea of a rational system of the practice of medicine. But for originality, either he came too late, or he was void of its elements entirely. The whole Pinellian classification of fevers on which M. Andral pronounces his extravagant but misplaced panegyric is derived from two sources,—the First Lines of Cullen, and the Pyretology of Selle. To the former, he is indebted for pathology, nosological distinction, and description, and from the latter, he has taken his nomenclature, and several of his most absurd distinctions. System-making, however, and authorship are difficult tasks; and since the time of Livy, who set the example with regard to Polybius, it has been the fashion for the last aspirant for reputation to abuse his predecessor most heartily, and at the same time to borrow from him most liberally. Others of greater ingenuity, while they pass him with some cold sneer or careless remark, scruple not to preserve all the essential characters and peculiarities of his work, and trust to the ignorance or the indolence of readers for escaping detection. It is curious to remark, that Pinel, with all his excellences, has actually followed both courses as to Cullen; and while he has the merit of giving the French schools a pretty just view of the principles and practice of the Scottish Hippocrates, he has either not acknowledged his obligations to the original, or has done it in such terms as if he had incessant occasion to rectify and improve.

From Pinel, M. Andral passes to Broussais, to whom he ascribes the merit of localizing fevers, in other words, of tracing the febrile disorder to some local lesion. This, and the corresponding practice of combating the local affection by suitable treatment, he admits to be eternal and immutable truths in the doctrine founded by M. Broussais. But he maintains that the theory of this physician fails the moment he attempts to specify the particular lesion, or to restrict it to a certain organ or region of the human frame. He then proceeds to give an exposition of what the present state of knowledge justifies the pathologist to believe in regard to fever.

The cause of every *essential* fever, he maintains, may be traced to some change either in the solids, that is, we presume, in the organic tissues, or in the blood. Though essential fevers, therefore, he argues, are not general diseases, in always having a point of origin, they may become so in this respect, that, being seated occasionally in the nervous centres, or in the blood, they must constitute a disorder of universal presence, in so far as blood and nerves are everywhere diffused through the system. He then arranges them in three families, according to the part of the frame originally affected,—the first depending on primary change in the nervous centres, the second on the lesion of a solid, and the third on a modification of the blood. The fevers of the first family are distinguished by that assemblage of symptoms which attend the adynamic, and especially the ataxic fever of M. Pinel, or the *typhus* and *synochus* of Cullen. In the second family, the solid most usually affected is said to be the alimentary canal, though it may also be the skin, the lung, the liver, the kidneys, the heart, the vessels, the uterus, the ear, the prostate, &c.; and that these parts may be disordered without affection of the alimentary canal, M. Andral possesses the means of demonstrating by anatomical evidence, and even in these instances it is impossible to trace any proportion between the severity of the textural lesion, and the apparent intensity of the external symptoms. In the third family M. Andral restores the forgotten humoral pathology, and we think not under good auspices. He thinks *inflammatory* fever often depends upon no other cause than the excessive *richness* of the blood in fibrine; and conversely, that blood *impoverished*, either accidentally or naturally, is often connected either with mucous fevers, or with those distinguished by sudden depression of the powers. Blood not sufficiently *depurated*, he informs his readers, is the certain cause of *urinous* fever, and the probable one of certain bilious fevers; and, lastly, blood which has received deleterious substances, animal or vegetable miasms, is the primary seat of proper typhus. It is almost superfluous here to remark the faultiness of this division, in representing the same fever *(typhus)* to originate at once from disorder of the nervous centres, and some morbid change in the constitution of the blood. Nor is it requisite to enter into a serious examination of the truth of this chemico-humoral hypothesis, which may form a very good plaything for the system-builder, but can never attract the serious attention of the observant physician. It is idle to say that the solidist pathologists never went the length of denying the morbid state of the fluids in disease, and that they merely contended against erecting it into a universal cause of all maladies, while it was more consonant to reason and just observation to say that the solids, or rather the organic textures, were first affected in the greater proportion of disorders. We have already taken the liberty of recommending one English work to the attention of M. Andral, in order to rectify some of his views. We may add, that in another, the Institutions of Medicine by the same author, and the *Conspectus* of his successor, Dr Gregory, he will find very correct, and in our estimate very instructive statements of the comparative influence of the solids and the fluids in the production of disease.

In the second section, on lesions of the supra-diaphragmatic portion of the alimentary canal, the acquired lesions are considered in the first chapter, and the congenital in the second. Among the first we find the *diphtheritis* or corial inflammation of M. Bretonneau, *œdema* of the uvula and pharyngeal membrane, induration, often called *scirrhus* but apparently hypertrophy, enlargement or hypertrophy of the tonsils, tubercular deposits and concretions of the tonsils, and observations on the soundness of the opinion which admits gangrene of the mouth and throat as a common affection. These M. Andral regards as examples of *stomatitis* or *pharyngitis*, with the formation of grayish or blood-stained false membranes. Even the *cancrum oris* of infants he regards as a variety of *stomatitis* terminating in gangrene. The morbid states of the œsophagus and its mucous membranes, for instance hyperemy, induration and thickening, ulceration, perforation, and tubercular deposits, are briefly noticed.

The pathological history of the congenital lesions consists chiefly of a series of names, some borrowed from the German authors, others of new construction, with corresponding definitions. Thus *astomia*, the want of mouth, is the normal state of the fœtus for the six first weeks of its existence ; and its persistence to the time of birth is the effect of a check given to the process of developement. *Atelostomia*, misdevelopement, comprehends *aprosopia*, want of face ; *ateloprosopia*, imperfectly formed face ; *agnathia*, want of lower jaw ; *atelognathia*, imperectly formed lower jaw; *acheilia*, defect of lips; *atelocheilia*, badly formed lips, hare-lip, fissured palate; *aglossia*, want of tongue; *ateloglossia*, incomplete tongue; *microglossia*, preternaturally small tongue; *macroglossia*, unusually large tongue, and bifid tongue, with all the numerous deviations incident to the teeth, either in number, shape, size, position, or formation. On the latter subject M. Andral communicates a good deal of interesting matter.

The theory of malformations of the pharynx and œsophagus depends on the same general principles on which malformations in general are understood. But the natural mode of formation is imperfectly ascertained. Absence of the pharynx is found chiefly in acephalous fœtuses; and in others this cavity is found distended into a sac, or its mucous membrane is protruded in the manner of a hernia through the muscular parietes. As to the œsophagus, its upper extremity may terminate in a blind sac, or it may be a chord without cavity, or the cavity may be only a little above the insertion into the stomach, or by the want of its anterior wall, it may communicate directly with the windpipe. In other instances in which the tendency to excess is conspicuous, part of this tube may be double, as in cases in which the parts are more or less double throughout. Its misplacement is remarked only when the viscera generally are transposed.

The lesions of the apparatus of circulation are treated so much in the same order and style, that after directing the attention of our readers to the following tabular view, we shall take the liberty of passing over the rest of this part in a very cursory manner. For this also there is an additional reason in the circumstance, that M. Andral goes over much the same ground which has been already trodden by Baillie, Corvisart, Farre, Burns, Kreysig, Laennec, Meckel, and Bouillaud, and necessarily reproduces many of the subjects which have been investigated at sufficient detail by these authors. It is almost superfluous to mention, that the names of the authors not French are passed over in silence; and no one would imagine that either English or Germans had studied any of the diseases or malformations of the heart and blood-vessels.

Lesions of Circulation.

Diseases of the heart.	Lesions of Nutrition.	I. Opposing the free circulation through the heart;—the different forms of hypertrophy, and atrophy.	A. Obstacles in the orifice of the heart.
			B. ——— in the arteries.
			C. ——— in the capillaries.
		II. Not affecting the dimensions of the heart.	1. Induration.
			2. Softening.
			3. Solutions of continuity.

- Diseases of the heart.
  - Congenital Lesions of Nutrition.
    - 1. *Acardia*, heart wanting.
    - 2. *Atelocardia*, incomplete heart.
    - 3. Excessive developement.
    - 4. Misdirection.
    - 5. *Ectopia*, malposition.
  - Lesions of Secretion.
    - In the adipose exhalation.
    - In the perspiratory exhalation.
      - In the substance.
      - In the cavities.
  - Lesions in the blood contained in the cavities of the heart.
  - Lesions in the innervation of the heart.
- Diseases of Arteries.
  - Lesions in circulation.
  - Lesions in nutrition.
    - Enlargement of the calibre of arteries.
    - Contraction of arteries.
    - Obliteration of arteries.
  - Congenital lesions.
  - Lesions of secretion.
  - ——— innervation.
- Diseases of Veins.
  - Lesions in circulation.
  - ——— nutrition.
  - ——— secretion.
- Diseases of the Spleen.
  - I. Lesions affecting the matter of the cells.
    - Changes in consistence.
    - ——— volume.
    - ——— colour.
    - New productions.
  - II. Lesions affecting its fibrous tissue.
  - III. Causes and nature of alterations in the spleen.
- Diseases of the Lymphatics.
  - Lesions of the lymphatics.
  - ——— lymph.
  - ——— lymphatic ganglion.

From this table a tolerably just idea of the order in which Andral investigates his subjects may be formed. The accou of the lesions of circulation in the diseases of the heart is ve brief and rather meagre; at least it is, like most of the views this volume unsatisfactory, or giving no explanation either the origin or the termination, the developement or the tende the etiology or the consequences of lesions of the circulatio In the account of those of nutrition he arranges under the g neral and somewhat vague head, of lesions opposing the free c culation, not only the different forms of hypertrophy, gene and partial, eccentric and concentric, but atrophy, whether rect, or the consequence of the encroachment of a new grow in the substance of the organ, extenuation of its walls, a whatever diminishes either the cavities or the orifices, or enl ges the cavities and contracts the orifices.

Of *atelocardia* he gives the following varieties, illustrati the connection which subsists between the formation of the h man heart, and the normal structure of this organ in the se ral classes of the inferior animals.

a. One auricle, one ventricle, the pulmonary artery arising from the aorta.

b. Some traces of the septum of the auricles; one ventricle; the aorta and the pulmonary artery rising from it separately.

c. Imperfect separation of the auricles; rudiments of the septum of the ventricles, appearing at the apex, and extending progressively to the base.

d. Complete separation of the ventricles; imperfect separation of the auricles; the *foramen ovale* persistent. The last malformation often coincides with the absence or the imperfect developement of the ventricular septum.

e. Want of the right auriculo-ventricular orifice; persistence of the *foramen ovale* and *ductus arteriosus.*

In the foregoing series of malformations, it is not difficult to trace a curious correspondence with the different forms which the organ assumes in the several classes of the lower animals. Thus the *acardia*, which may be taken as the lowest point in the scale, is represented in insects by the absence of heart. The heart with one auricle and one ventricle is the heart of fishes. The next variety with traces of the auricular septum, one ventricle, and the aorta and pulmonary artery arising from it, corresponds with the heart of the lizards and crocodiles. The third variety again corresponds with the heart of several of the turtle family, in which the ventricles communicate.

Change of direction M. Andral distinguishes from change of position *(ectopia)* or displacement, which, indeed, constitutes a sort of hernia of the heart. In the latter case its situation may be so much altered as to place it in the region of the kidney, in that of the navel, in that of the palatine vault; or if the thoracic parietes are imperfectly developed, it may form a projection on the external surface of the chest, covered only by the common integuments. On this point M. Andral might have derived some good information from an ingenious thesis by Weese, published at Berlin in 1819.

The first sub-section of the second article on lesions of the perspiratory exhalation in the substance of the heart, is an extraordinary article. To this head M. Andral refers such heterogeneous affections, as the white spot, ossification in the cellular tissue, in the fibrous tissue, and in the muscular fibres, purulent deposits, tubercular deposits, scirrhus, encephaloid degeneration, serous cysts, and hydatids. Of lesions of the perspiratory exhalation into the cavities he has been unable to make more than purulent matter, pseudo-membrane, and albuminous deposite.

To illustrate the nature of the lesions of the blood contained in the cavities of the heart, he adduces the instance of the bloody clot, or polypus of the older authors, in the organization of which he is a firm believer; and endeavours to explain in this manner the formation of the vegetations which arise from the inner surface of the heart, and especially from the valves,—a theory which he labours to establish on the circumstance of the various shades of colour which these growths present in different stages of their progress. He admits, nevertheless, that they may be the consequence of inflammatory exudation, when the inner membrane of the heart becomes the seat of that process,—a much more probable mode of explaining their origin.

On the subject of diseases of the arteries, in the first and most interesting chapter, the author inquires into the nature of the red tint so often found inside the arteries, and which has been admitted by Hodgson, and all the best subsequent authorities, to be the result of the tinging or dyeing property of the colouring matter of the blood. Upon this point it appears that the French pathologists have perplexed themselves not a little, and are somewhat at variance. The researches of MM. Bertin and Bouillaud showed that the red colour does not depend on putrefaction, and that in many instances it is a cadaveric process, depending, however, on the tinging property of the blood. From the researches of MM. Girard, Dupuy, and the author again, on the bodies of horses, cut off by an epizootic in 1825, it appears to be connected with disorder of inner membrane of the heart and arteries. By MM. Rigot and Trousseau, on the contrary, who have inspected the arteries of many horses after death, it is maintained that this redness is not observed immediately after death, but may be recognized if inspection is delayed for some hours. Amidst these discordant statements, M. Andral remarks, that the inspections of the latter observers took place the year after the epizootic, and when it was no longer in existence, and that this redness of the inner arterial membrane, though seen during the period of the epizootic, was no longer cognizable after its subsidence, and hence infers that it was the result of a morbid state, which he thinks might be either an irritation of the arterial parietes, or dependent on some morbid state of the blood of the sickly animals. He then states, that the uniform redness of the inner surface of the arteries may, in some circumstances, be the result of active hyperemy. Upon some other occasion we may probably recur to this subject.

Though the subjects of the other subdivisions of this section are truly important, we feel that we cannot with justice occupy the valuable time of our readers with notices necessarily meagre and imperfect, of subjects which have been treated in a manner so masterly by Jones, Hodgson, and other British pathologists.

From this part of the volume, inded, we fear the English reader will be unable to derive any new information; and we may therefore, without scruple, proceed to the section on the spleen, which M. Andral classes with the organs subservient to circulation.

This he appears to have done in consequence of its intimate structure, making some approach to that nominated *erectile.* From personal researches made with the view of elucidating the structure of this organ, M. Andral infers, nearly as Ribes had already done, that it consists of three elementary constituents;—1*st*, Fibrous tissue arranged externally like a capsule, and divided internally into multiplied filaments, among which the blood is effused; 2*d*, A vein which throughout communicates with the cells by large orifices perforating its walls, and the interior of which communicates with the interior of the cells; and, 3*d*, An artery distributed to the fibrous walls of the latter, but the ultimate termination of which is unknown. From this it follows that the spleen consists of two parts, a containing, which is fibrous, or rather fibro-vascular, and a contained, which is blood. It would be unreasonable to doubt that this peculiarity of structure exercises a decided influence on the character of the morbid alterations incident to the spleen; but whether it be that the normal arrangement is still imperfectly ascertained, or that M. Andral has been unable to trace the connection between this and the pathological conditions, certain it is that it does not appear so luminously as might be expected. The changes in consistence are ascribed by the author to changes in the consistence of the blood contained in the splenic cells, so that softening and induration of the spleen are simply soft and hard states of the splenic blood. Whatever opinion may be entertained on the justice of this view as applied to the former state, it is clearly inapplicable to the latter. No one who has inspected the section of an indurated spleen could ascribe that induration to the state of the blood only. The opinion which ascribes change of volume also to the presence of the unusual quantity of this fluid, may be well-founded when the enlargement is temporary; but we know from actual inspection that it is erroneous in the more frequent cases in which this is of long duration. Change of colour is improperly made to constitute a separate mode of disease, for if it exists alone, it is scarcely morbid, and when morbid it is conjoined with change in consistence, in volume, and in density. In illustrating the nature of the causes of splenic disease, he properly adduces the great and sudden changes which the organ is seen to undergo in experiments on animals, and which are probably dependant on the general disturbance of the circulation. The presence of disturbance of this description in the paroxysms of ague may doubtless afford an illustration, if not a positive proof of the manner in which the spleen becomes diseased in that complaint. On other topics a lack of information either new or useful is conspicuous, and the pathological reader will observe with astonishment, that to the practical observations of Pemberton,[*] the instructive researches of Bree,[†] and the excellent monograph of Grottanelli,[‡] M. Andral is equally a stranger.

On the subject of the diseases of the lymphatic system, we have only to remark, that the instance of obstruction of the pelvic lymphatics, or rather of the thoracic duct from disease of the pelvic bones, which he quotes from Soemmering and Lauth, a little research might have shown, was originally published by Cheston in the 70th volume of the Philosophical Transactions in 1780.

Of the diseases of the respiratory organs the following table shows the arrangement:

I. Lesions of the air-tubes.	Lesions of	Mucous membrane,	Circulation, Nutrition, Secretion,
		Submucous tissues,	
		Changes in the dimension of the air-tubes.	
II. Diseases of the pulmonary parenchyma.	Lesions of	Circulation,	Hyperemy of the lung, Anemy of the lung,
		Nutrition,	Hypertrophy of the lung, Atrophy of the lung,
		Secretion,	Purulent secretions, Tubercular secretions,
		Innervation,	
III. Diseases of the thyroid gland.			

The diseases of the organs of secretion which come next are arranged in the following order:

Organs of perspiratory secretion.	Lesions of	The secreting tissue. The secreted fluid.	
Glandular secretion.	The liver and its appendages.	1. Hepatic parenchyma.	Circulation. Nutrition. Secretion.
		2. Excretory ducts. 3. Fluid secreted.	
	The urinary organs.	The kidneys.	Circulation. Nutrition. Secretion.
		The excretory passages.	The calyces, pelvis, and ureters. The bladder. The urethra. Changes in the urine.

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[*] Treatise on various Diseases of the Abdominal Viscera. By Christopher Pemberton, M. D. &c. London, 1806.

[†] Medico-Chirurgical Transactions, Vol. III. and IV. London, 1806.

[‡] Ad Acutæ et Chronicæ Splenitidis in humilibus præsertim Italiæ locis consideratæ eidemque succedentium Morborum Historias Animadversiones. Auctore Stanislao Grottanelli, Phil. Med. et Chirurg. Doct. &c. Florentiæ, 1821. 8vo.

The account of the morbid changes incident to the organs of this division, is, upon the whole, not positively bad. The distinctions, nevertheless, are often too refined, and the principle of the division is not always accurate. What, for instance, can be more repugnant to the spirit of sound physiology than to place the morbid changes incident to the urine under the head of diseases of the excretory organs? If there be one point in the physiology of the urinary secretion better ascertained than another, it is, that the condition of the urine depends either directly upon the condition of the kidney and its vessels, or upon the state of those organs, especially the chylopoietic, which influence the state of the kidneys. The conductors and recipients of the secretion have in general little connection with its healthy or morbid conditions. We must, nevertheless, not forget, that M. Andral has taken the pains to make his countrymen acquainted with the results of the valuable researches of Dr Prout. On the important subject of the disorders of the bladder and urethra, the information is well arranged, but meagre and common-place to the English reader who is acquainted with the writings of Sir Everard Home, John and Charles Bell, Abernethy, and the numerous authors who have lately written on the pathology and treatment of urethral diseases.

Upon the same principles nearly, are arranged and treated the diseases of the organs of generation, and those of *innervation*, that is, of the brain, spinal chord, and nerves. In the former, M. Andral gives a sketch not only of the disorders of the male and female organs, but of the anormal deviations of structure observed in the fœtus. Of the diseases of the latter class of organs the account is full, and, though not free from the same faults with which the work in general is tinged, by no means uninstructive. Under the head of *lesions of circulation occurring in the nerves*, a good example of the yellow-coloured degeneration, denominated *cirronosos* by Lobstein, is described as affecting the Gasserian ganglion and the three divisions of the fifth pair of nerves, and giving rise to epileptic seizures and insensibility and immobility of the eye, with insensibility of the nasal mucous membrane of the affected side. Various other lesions, as hypertrophy and atrophy, are described with considerable interest; and upon the whole this may be justly said to be the most interesting if not the best part of the volume.

This may be said to be a reason for entering more at length into the consideration of the several subjects here discussed. It must be observed, however, that though the matter is more interesting than in other parts of the work, it is by no means new; and we feel that we cannot with propriety occupy the time of our readers any longer on this subject. Here, therefore, we must conclude our observations, and we do so with a feeling of regret that we have had so frequent occasion to censure what we hoped we should have reason only to eulogize. The present work is indeed a melancholy example of the facility with which, with all the best means and sources of information, a very indifferent book may be composed. It comes not within the object of these observations to explain the cause of this failure, for a failure it must be admitted to be. To say that it is the principle upon which it is arranged, is only to pronounce a censure on the judgment of M. Andral in chusing a mode of arrangement which is incompatible with the formation of a useful work. It is manifest, nevertheless, that this is one cause why so much physiological matter, not always of the most reputable kind, is introduced throughout. In short, so much is this the case, that the work is really mis-named, and has a much more just claim to the title of *pathological physiology* than that of pathological anatomy.

ART. II.—*Recherches Anatomiques, Pathologiques, et Therapeutiques sur la maladie connue sous les noms de Gastro-entérite, &c. An Inquiry into the Pathology and Treatment of the disease termed Gastro-enteritis, or Putrid, Adynamic, Ataxic, Typhoid Fever, &c. compared with other common acute diseases.* By P. CH. A. LOUIS, M. D. &c. &c. Paris and London, Bailliére, 1829. 2 vols. 8vo.

IN 1813 M. Petit, aided by M. Serres, then one of his assistants at the Hotel-Dieu of Paris, published an account of the pathology, symptoms, and treatment of a disease of the intestines, which came under his notice during his clinical courses at that hospital, and which had till then been confounded with typhus or adynamic and ataxic fever.* This disease, which he found to be extremely frequent at Paris, and which has continued ever since a common disorder in the hospitals there, is characterized by all the symptoms of the slower forms of continued fever, but depends, according to the researches of MM. Petit and Serres, on certain organic changes in the mucous membrane of the lower part of the small intestines, and on the enlargement and subsequent suppuration of the corresponding glands of the mesentery. By these authors the disease was termed *Entero-mesenteric Fever.*

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* Traité de la Fièvre Entéro-mésenterique, observée, reconnue et signalée publiquement à l'Hotel-Dieu de Paris dans les années 1811-12 et 13, par M. A. Petit, &c. composé en partie par E. R. A. Serres.

Since the publication of M. Petit's treatise, this affection has been noticed by various authors. A correct delineation of it may be seen in *Dr Burne's* Treatise on Adynamic Fever;[*] an excellent description of it has been also given by *Dr Bright* in his Hospital Reports;[†] certain forms of it, which end in perforation, were described some years ago by *M. Louis*, the author of the work about to be analyzed;[‡] a year earlier some good cases were related by *M. Andral* in one of his pathological works;[§] at a still earlier period some cases were published by *Dr Abercrombie*,[||] who has also again noticed the disease in his late work on the pathology of the stomach and intestines;[¶] and so early as 1803, several examples of it were described by *M. Prost*.[**] But the pathologist, who appears to have investigated the subject most carefully, who traced the disease to its real seat, the isolated and conglomerated mucous crypts of the intestinal canal, and who is said by his pupils to have recognized its exact nature so early as 1813, is *M. Bretonneau* of Tours.[††]

Various opinions have been formed by these and other authors as to the relation which the disease of the intestines bears to the symptoms of general fever which occur along with it. British authors who have described it, consider it as sometimes an idiopathic disease imitating fever, and at other times as a secondary affection occurring during typhus or adynamic fever, just as ulceration of the colon, pneumonia, and other local phlegmasiæ are known to do. The French authors, on the other hand, consider it as the pathological cause of such fevers, and deny that it is a secondary affection. Not a few of these, among whom may be mentioned M. Louis, and we should suppose also M. Bretonneau, maintain that there is no such thing as a case of typhoid, adynamic, or ataxic fever, without this disease in the intestines. It is one of the diseases, in short, by which an attempt is made by the French pathologists to disprove (with what success will presently be seen,) the existence of Essential fever, or fever independent of local disorder.

By far the most elaborate work hitherto published on the subject is the treatise of M. Louis, of which some account will be given in the following pages.* It consists of three divisions. In the first, the author gives a very extended view of the state of the whole viscera of the body, as ascertained by inspection after death. In the second, he gives an equally detailed view of the symptoms referrible to the leading organs and functions of the body. And in the third, he dwells shortly on the treatment. In the two former divisions, while he endeavours to ascertain the appearances and symptoms which are essential to the disease, he also attempts to discover what symptoms and organic derangements are liable to occur as secondary to the intestinal affection; and with this view, compares the secondary symptoms and appearances he observed, with those remarked in other acute febrile disorders. His statements are drawn from accurate notes kept of forty-six dissections, and eighty-eight cases of recovery.

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[*] A Practical Treatise on the Typhus or Adynamic Fever, 1828.

[†] Reports of Medical Cases, 1827, p. 188.

[‡] Archives Gén. de Méd. 1823, *Janvier*.

[§] Recherches sur l'Anatom. Pathol. du tube Digestif, 1822.

[||] On the Pathology of the Intestinal Canal. Edin. Med. and Surg. Journal, xvi. 329, 1820.

[¶] On the Diseases of the Stomach and other Abdominal Viscera, 1828.

[**] La Médecine éclairée par l'ouverture des Cadavres.

[††] *Trousseau*, de la Maladie à laquelle M. Bretonneau a donné le nom de Dotinenthérite. Archives Gén. de Méd. x. 67, 1826. M. Bretonneau has not yet published a long-promised work of his own on the subject.

With respect to the affection which he considers the primary disorder, namely the induration and subsequent ulceration of the conglomerated mucous glands of the lower part of the ilium, it does not appear that much is added in the present treatise to the information formerly communicated by MM. Petit and Serres, and more particularly by M. Trousseau from the researches of Bretonneau. M. Louis' narrative of the cases and his general summary are, it is true, much more minute than those of his predecessors; but they contain no material novelty. On the other hand, his investigations abound in new results on an important point which M. Petit and M. Trousseau have passed over very cursorily,—the secondary affections which sooner or later become complicated with the primary disease. These affections are according to M. Louis' researches, extremely diversified, much more frequent than in any other febrile disorder, and a common enough cause of the disease going on to a fatal termination. His exposition of them will afford much subject for reflection, abounds in practical inferences, and is equally interesting whether we follow him in considering the disease primarily an intestinal affection, or adhere to the British doctrine that the fever is the essential disorder, and the intestinal affection one of its secondary concomitants. Nowhere in fact will the physician find a more complete account of the secondary phenomena of typhous or adynamic fever; and in many respects the statements advanced are both new and practically important.

The first volume of the work is entirely confined to the pathology of the disease. Here M. Louis first gives the details of the symptoms and morbid appearances in a great number of cases, arranged in three groupes, according as death took place between the eighth and twelfth days, between the sixteenth and thirtieth days, and after the thirtieth; and then he enumerates the morbid appearances found in every important organ, endeavouring to refer each appearance to the stage of the disease at which it is usually or solely formed. In the following summary, an attempt will be made to follow him in his progress throughout the body.

The *Pharynx* exhibited superficial ulcers in a sixth part of the cases dissected, and sometimes an abscess in the submucous cellular tissue. It was never found diseased in those who died before the fifteenth, and once only in a patient who died after the thirtieth day. Ulceration of the pharynx is a secondary disorder, to which there is a strong predisposition in adynamic fever, for it was found once only in seventy mixed cases of other acute diseases. The *Gullet* was superficially ulcerated in the same proportion and same description of cases as the pharynx, and never in other acute diseases. The pharynx and gullet were sometimes affected when the stomach was healthy.

The *Stomach* was sometimes enlarged in capacity. Its mucous membrane was very generally altered. Softening with attenuation, either continuous or in streaks, was met with in a fifth part of the cases, and very rarely in those who died after the twenty-seventh day. The softening in one case affected the cellular tissue, and sometimes it was attended with redness. Ulceration, distinguished from the last appearance by the small extent and sharp border of the eroded parts, was much less common, having been witnessed in a twelfth part only of the cases. Simple softening without attenuation was observed in nearly a third part of the cases, and in four instances it was universal. Mammillation of the mucous coat was present in two-sevenths of the cases, and in two-thirds of these it occupied almost the whole surface of the stomach. It very rarely occurred in the bodies of those who died after the twenty-seventh day. It was sometimes complicated with the other derangements of structure already mentioned, and almost always with redness, grayness, or bluishness of the mucous coat.—All the appearances now described were frequently met with after death from other acute febrile diseases of some standing, but rather less frequently than after adynamic fever. Hence M. Louis concludes, and it is an inference of much importance, "that in all acute diseases which occasion a febrile movement of some duration, the mucous membrane of the stomach is apt to become, at a variable period of the disorder, the seat of an organic lesion, which differs in severity according to the predisposition of the subject, and which may accelerate death, or even be its principal cause." The softening, either with or without attenuation, which he so frequently observed, he considers not to be an inflammatory affection.—In thirteen of his forty-six inspections, it is material to add, the stomach was quite healthy in colour and consistence. As in these cases all the symptoms of adynamic or typhous fevers were present, the statement now made is a sufficient answer to those modern pathologists, who would have us believe that a *gastrite* is the invariable cause of the symptoms which constitute continued fever.

The *Duodenum* was in a few instances red, or slightly ulcerated, or softened in its villous coat; but the same appearances were found as often in acute diseases generally.

The remaining *Small Intestines* were distended with air in more than a third of the cases; generally contained a good deal of mucus, commonly also much liquid bile, and sometimes mixed bile and mucus tinged with blood; and the colour of the mucous coat was commonly white in the rapidly fatal cases, red in a third part of the cases both short and prolonged, and gray in many of those who died after the twentieth day. Hence it is probable that the gray colour occurs in the course of the passage of the redness of inflammation towards the healthy colour of the membrane. Softening of the mucous coat occurred in a fifth of the whole, and particularly in the cases that proved fatal at an advanced period. It does not appear to be an inflammatory affection.—In every instance the oval patches of conglomerated mucous crypts in the intestines were more or less diseased. The author has described the progress of this disease with extreme minuteness. But, as his description does not differ in substance from the account formerly given in this Journal of the description of the same object by Dr Bright,* it appears unnecessary to follow M. Louis throughout his details. The early stage of the organic alteration consisted in redness, thickening, and softening of the Peyerian glands, or conglomerated mucous crypts, sometimes with the deposition of a friable matter in the submucous membrane. At a more advanced stage the surface of some was ulcerated, while others were bluish or grayish, and less thick. The latter appearance seemed to indicate a retrograde course. At a still more advanced stage the ulceration spread deeper and wider, and the worst ulcers had a ragged, fungoid margin. An appearance not uncommon in old cases was a depression where an ulcer had existed, covered with a fine transparent pellicle, continuous with the submucous cellular tissue. These were evidently cicatrices. The ulceration was always farthest advanced nearest the cœcum. Ulceration was rarely observed in those who died before the fifteenth day. The diseased state now described was attended in some instances with redness, enlargement, and, more rarely, ulceration of the isolated mucous crypts or glands of Brunner. In other acute diseases the redness, grayness, and softening of the intestinal mucous coat, as well as the enlargement of the glands of Brunner, were found not unfrequently, though not so often as in cases of adynamic fever; but the thickening and ulceration of the elliptic patches of conglomerated crypts were never met with. "Hence, as this last alteration of structure was present in every case of adynamic or typhoid fever, and never in other acute diseases; as it followed in all cases the same law as to progress, being found nearly in the same state of progress at the same period of disease; as it was seen completely formed in eight days after the commencement of the fever; and as it was sometimes the only organic derangement in the body,—it must be considered not only as peculiar to typhoid fever, but likewise as forming its anatomical character, in the same manner as tubercles form that of phthysis, whatever be the cause which developes them."—P. 123.

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* Vol. xxx. p. 375.

The *Colon* was much distended with gases in three-fourths of the cases; and this state occurred most frequently in those who died between the twentieth and thirtieth days. It was not attended with attenuation of the coats. The colour of the mucous membrane was in a third of the cases natural, in the rest red, gray, or yellow. Its consistence was natural in nearly the same proportion. In the remainder it was generally or partially softened, and the softening was sometimes accompanied both with thickening and redness. The mucous crypts were enlarged in a sixth part of the cases; hard elevated patches were found in half that proportion; and ulcers were seen in a third of the whole cases. The ulcers were seldom numerous or large, were commonly situated in the cœcum, and were most frequently met with in the cases which proved fatal at a late period.—All the appearances now enumerated were also found in mixed cases of other acute diseases besides fever, with the exception of the hard, elevated patches, which were probably analogous to the indurated oval plates in the lower part of the ileum, and are peculiar to the typhoid fever. Gaseous distension of the colon was much less frequent in other acute diseases than in typhoid fever, as it did not occur in more than a twenty-third part. It is therefore an important character of the disease under consideration.

The *Mesenteric glands* were diseased in every case of adynamic or typhoid fever without exception; and their state of progress generally corresponded with the progress of the oval patches in the intestine opposite. In cases which proved fatal between the eighth and fifteenth days, the glands were simply enlarged a little, pale-red and friable. Where the oval patches were much enlarged and approaching the stage of ulceration, the mesenteric glands were proportionately large and dark-red. Where the oval patches were ulcerated the glands contained pus; and where the patches were grayish or violet as if retrograding, the enlarged glands were also of the same colour. But although the glands thus appeared to become diseased by sympathy with the disease in the intestine corresponding with them, in some instances also they were enlarged where the conglomerated mucous crypts in the opposite part of the intestine were healthy.—The *Mesocolic glands* were also commonly enlarged when the mucous coat of the colon presented hardened patches, or ulcers, but likewise also occasionally when that membrane was quite healthy. The *Lymphatic glands around the biliary ducts* were enlarged in two cases only. The lymphatic glands of the stomach were also sometimes enlarged; but their state was seldom attended to; those of the groins and loins in two cases; those of the axilla and neck in twelve instances. In other acute diseases such as peripneumony, small-pox, scarlatina, and erysipelas, the mesenteric and mesocolic glands were found only six times slightly red and enlarged, but never so extensively altered in structure as in typhoid fever; the glands around the biliary ducts were never diseased; and the cervical glands only in four instances. Hence the diseased condition of the glandular system, more especially of the intestines, is very nearly, if not entirely peculiar to the typhoid fever. It was not without justice, therefore, that Petit termed this disease, as it occurs in France, *Entero-mesenteric fever;* yet M. Louis nowhere takes notice of the name thus given to it, while at the same time he expresses himself at a loss how to name it, and actually finishes his work without inventing a designation.

The *Spleen* was in its natural state in four cases only of typhoid fever. In three-eighths of the cases it was enlarged to more than three times its usual volume; in three-fourths it was softened, in a fifth-part of these to such a degree as to be reduced to a pulp with the greatest ease; and its colour was also generally changed to a dark bluish-black or reddish-black. As this diseased state was universal in those who died at an early period, and was less and less frequent in the more advanced stages, it was probably towards the beginning of the disease never absent. The softening was not of an inflammatory nature; for it always affected the whole organ, its degree was the same in every part, the peritonæal membrane was never inflamed, and pus was never formed,—a conjunction of characters inconsistent with inflammation as it appears in other organs. Softening of the spleen occurred much less frequently in other acute diseases, namely in little more than a fourth part. This affetion therefore seems, like softening of the stomach, to be connected with the prevalence of a febrile state of the body; but it is much more apt to occur during the pyrexia of entero-mesenteric fever, than in any other.

The *Liver* was in a very few cases somewhat enlarged in volume, in a few more red in colour than natural, in half the cases unusually pale, dry, and more or less softened, occasionally to a very great degree, so as to be exceedingly friable. The softening was remarked at all stages of the disease. It was nearly as frequent in other acute disorders besides typhoid fever. It was evidently not an inflammatory affection, for the same reasons with those assigned for the non-inflammatory nature of softening of the spleen.—The *bile* was unusally fluid, and reddish or greenish in one-half of the cases, and unusually abundant in three-fifths of these. In others it was very dark and viscid. This state of the bile corresponded with softening of the liver in three-fifths only of the instances of that affection. The mucous membrane of the gall-bladder was thrice found inflamed. The bile and gall-bladder were much less frequently altered in other acute diseases than in typhoid fever.

The *Kidneys* were darker and softer than usual in about a sixth of the cases both of typhoid fever and of other acute diseases. The *Bladder* was much distended with urine in a fifth of the cases, red in nearly as many, and once slightly ulcerated. These appearances were less common in other acute diseases besides typhus.

The *Pancreas* was darker-coloured than usual in a few cases of typhus. The *Parotid* gland was twice only found inflamed. The *Genital organs* were rarely or never affected, either in the male or in the female.

The *Pericardium* was never diseased in typhoid fever, and in a seventh only of the cases contained serum. The *Heart* was in half the cases more or less softened, and in two-thirds of these to a great degree, so that it retained any form given it by the fingers, and was very easily torn. It was also in most of these cases thinner than usual, dark in colour, and dry. This affection was uniformly greatest in the instances of rapid death, and, as will afterwards be seen, was invariably conjoined with a hurried, fluttering, irregular, feeble pulse. It was confined to the left ventricle in two cases, was universal in the remainder, but commonly greatest in the left side of the organ. The cavities of the heart sometimes contained fibrinous concretions; but in all cases of advanced softening of its parietes, the blood was in loose clots, without separation of fibrin, and in the most advanced cases it was fluid, contained air-bubbles, and was present in the heart in very small quantity. This softening of the heart is evidently an analogous affection with the softening of the liver and spleen already noticed, and for the reasons mentioned under these two heads is not of an inflammatory nature. It is remarkable that the other muscles of the body were never similarly affected. Softening of the heart occurs in other acute diseases, but much less frequently than in typhoid fever. The internal surface of the *Aorta* was frequently red, and very rarely except when the heart was softened.

The *Respiratory organs* were frequently diseased. The *Epiglottis* was thickened and covered with a pseudo-membrane in two cases, and in seven others thickened, red, and partially eroded. This affection was never met with in those who died before the fifteenth day. It was very rarely found in other acute diseases besides typhoid fever.—The *Glottis* was contracted by œdema in two cases of typhus. The *Larynx* in three cases was lined by a pseudo-membrane, and superficially ulcerated in another. The *Trachea* was healthy in every instance. The *Lungs* were in a third of the cases nearly or altogether healthy; partially hepatized in two-fifths, especially towards the lower extremity of the inferior lobes; gorged or in the first stage of inflammation in three-sevenths; checkered with a few purulent particles in one instance; and containing dispersed tubercles in five others. All these affections of the lungs appear to have been nearly as frequent in other febrile affections as in typhus. The *Bronchial tubes* generally contained some mucus, which in one instance only of typhus was puriform. The *Pleura* presented traces of recent inflammation in two cases only, namely a delicate pseudo-membrane in one, and a sero membranous effusion in another. In two-fifths of the cases there was clear serum effused, which in a fourth only of these exceeded a pound.

In all the organs already noticed morbid appearances it appears were seen more or less frequently; and this often happened when there were no corresponding symptoms during life. In the *Brain* and its *membranes*, on the other hand, the morbid appearances were slight and equivocal, although decided symptoms of derangement of the cerebral functions were seldom absent.—In one case the inside of the *dura mater* presented a thin pseudo-membrane; in four there was a little fluid between the dura mater and arachnoid; in more than half the cases the *sub-arachnoid cellular tissue* was infiltered with serosity; and in a half of the cases the *pia mater* was injected with blood. The injection of the pia mater was greatest in the rapidly fatal cases, the effusions in the slower cases. In three-sevenths of the cases the *cortical substance* of the brain was redder than usual; in six-sevenths the *medullary matter* was injected; and both these appearances were most striking when death took place at an early stage. M. Louis does not consider the injection of the cerebral substance inflammatory, as it was universal, of the same degree throughout, and unattended with change of consistence. In six cases the density of the brain was uniformly increased a little; in five uniformly diminished. Neither of these states could be considered inflammatory, as the change was uniform throughout the whole brain; and neither could be considered as bearing any relation to the cerebral symptoms during life, such symptoms being almost always present, while, on the contrary, the organic changes in question were rare. The *Cerebellum* was even more frequently than the brain in a natural state; and the *Pons varolii* and *Spinal chord* were also almost always free even of redness. The *Ventricles* of the brain often contained a little serum, which was more abundant in slow than in rapid cases. In other acute diseases, including chiefly cases of peripneumony, pleurisy, peritonitis, small-pox, and erysipelas, the alterations of structure now mentioned were found quite as frequently as after typhoid fever. This fact shows how little support morbid anatomy gives to the opinion that the pathological cause of continued fever is inflammation of the brain.

Among the appearances found in the *Integuments* may be mentioned lividity, which was common, yellowness which occurred in a few instances, eschars formed by sloughing of parts subjected to pressure, induration of the skin and infiltration of the subjacent tissue where erysipelas had prevailed, and swelling, redness, attenuation, ulceration, and sometimes total destruction of the skin where blisters had been applied. Ulceration of blistered surfaces seems to have been more common in typhoid fever than in other acute diseases; but the remaining appearances now mentioned were equally common in both.

On reviewing the succinct account now given of the morbid appearances in the disease under consideration, it appears that, according to M. Louis, the primary affection is the enlargement, induration, and ulceration of the oval patches of agglomerated mucous crypts in the small intestines; and that the secondary affections which are apt to be combined with it are, in the earlier stages, enlargement and subsequent suppuration of the mesenteric glands, and a peculiar softening of various internal organs, the spleen, the liver, and the heart, and in the later stages softening, inflammation, and ulceration of all the mucous membranes, inflammation of the lungs, and gaseous distension of the colon. A singular circumstance was the great rarity of disease in the serous membranes, contrasted with the exceeding frequency of disease in the mucous membranes.

M. Louis concludes his description of the morbid appearances with a short notice of the explanation they furnish of the several causes of death. In two-fifths of the cases he considers the state of the oval patches of the small intestines was inadequate to account for death. In these the real cause was one or other of the secondary disorders. In three death was occasioned by phlegmonous erysipelas, in two by a recent arachnitis, in two by inflammation of the air-passages ending in fibrinous effusion, in one by œdema of the glottis, in two by suppuration of the submucous cellular tissue of the pharynx, in two by hepatization and gorging of the lungs, in two by the same disease, together with softening of the mucous membrane of the stomach, in two by extensive softening and attenuation of that membrane, and in two by a complication of secondary disorders in the stomach, lungs, spleen, and heart.

The greater part of the second volume of this work is occupied with a detailed account of the symptoms; which were ascertained by a careful classification of all phenomena referrible to derangement in any organ or function, and by an inquiry into their comparative frequency in typhoid fever, and in other acute diseases. This department of the work may be discussed here in a few words.

Diarrhœa was very rarely absent, and often present from the very beginning. The stools were commonly yellow, sometimes like coffee-grounds, occasionally sanguinolent, never mucous, and they generally amounted to four, six or eight in the course of the day. *Abdominal pain* was present in every fatal case, and in by far the greater part of the more severe of the cases which were not fatal. It was very often present from the very beginning, was commonly of the nature of tormina, sometimes, however, rather a sense of weight, and not unfrequently excited only by pressure. It seldom extended over the whole abdomen, generally was confined to the iliac fossæ, or hypogastrium, but often also extended to the umbilicus. *Flatulent distension* of the abdomen was a very common symptom, particularly in advanced cases, and very rarely occurred in other acute diseases besides typhoid fever. *Pain in the epigastrium* was common, but in fatal cases was far from being always connected with a diseased state of the mucous membrane of the stomach in the dead body, as the followers of Broussais imagine. *Nausea* was also very common, and, like epigastric pain, often continued till near death, though the stomach was not found diseased. Even these two symptoms not unfrequently occurred together without the stomach being found diseased. *Bilious vomiting* was another common symptom of the same class; but when this occurred in conjunction with epigastric pain, M. Louis never failed to discover in fatal cases softening and attenuation of the mucous membrane of the stomach. This is an important point of diagnosis. Softening with attenuation, however, it must be remembered, occurred often enough without any such symptoms.

The *Tongue* was often healthy or nearly so; frequently, however, red on its edge, or dry on its point and centre, sometimes altogether dry, brown, and encrusted; more rarely thickened, cracked, ulcerated, or covered with an abundant, white, pulpy coat. These states of the tongue bore no relation whatever to the state of the mucous coat of the stomach. They must be considered as merely a part of the numerous secondary disorders of the mucous membranes in this disease,—as an independent affection, by no means arising from or connected with disease in the stomach. The thick, white, pulpy coat sometimes remarked on the tongue occurred without any sign of inflammation of the organ. Hence it may be inferred that the secretions may become excessively morbid *without inflammation*,— a fact which some modern pathologists doubt. The *Mouth* and *Back of the Throat* were often similarly affected with the tongue and sometimes ulcerated; and this ulceration was by no means always connected with a diseased state of the stomach. The *Pharynx* and *Gullet* were also similarly affected, and then dysphagia was a common symptom. Dysphagia was sometimes purely a nervous affection, yet much more seldom than it is commonly thought to be in fever. In four-fifths of the cases where it occurred, M. Louis found it to be connected with actual disease.

Headach was very seldom wanting in the early stage of the disease, and was often present from the first. It disappeared as somnolency and delirium approached. *Somnolency* was rarely absent, sometimes appeared at the very beginning, more commonly, however, between the seventh and tenth days. It varied very much in intensity. It was always considered a very important symptom, both as furnishing by its intensity a criterion of the severity of the disease, and as being the probable cause of the absence of most of the signs of the secondary disorders found after death to have been present. *Delirium* was very frequent, and varied from slight wandering during the night only, to constant unmanageable furiosity and violence. It occasionally existed from the very first, but in most instances began between the eighth and sixteenth days. The cephalic symptoms bore no relation whatever to the state of the brain, the cortical substance of which was sometimes much injected when these symptoms were not high, and sometimes quite healthy as well as the rest of the brain, when the delirium was highest and somnolency deepest. Neither did these symptoms correspond with any other morbid derangement, except the disease in the ilium. In particular, they bore no relation to the state of the stomach, as some have pretended. Somnolency is considered by M. Louis a characteristic symptom of typhoid fever, as it was very rare in other acute diseases.

Spasms were often present, from slight subsultus to permanent contraction. The parts usually affected were the fingers, arms, face, neck, and diaphragm. Contraction of the arms, or of the cervical muscles, was an invariably fatal symptom. Spasms were very rare in other acute diseases besides typhoid fever. *Debility* was in general excessive, and in a degree quite out of all proportion to the febrile state. Sometimes it was so great, that the patient, though quite sensible, did not remove the arm when pinched. On the contrary, it was in a few cases so slight, that the patient continued to go about till a day or two before death. It was very rarely so great in other acute diseases as in typhoid fever. *Pains of the limbs* were generally present at the beginning, but disappeared as the somnolency advanced.

The organs of the senses were often affected. *Epistaxis* was very common, never critical, and most frequent and abundant when the diarrhœa was frequent. The *Conjunctivæ of the eyes* were often injected, but seldom in the first days,—corresponding in that respect with the other secondary affections of the mucous membranes generally, which were seldom disordered till an advanced period. There was sometimes pain in the eyes without redness; also dazzling or haziness of vision; and farther at times spasmodic contraction of the eyelids. *Ringing in the ears* was a very general early symptom; deafness was equally common at a later period, and it occurred often enough in fatal cases. Not unfrequently it was total in cases which terminated favourably. Pain of the ear was less common, and decided inflammation rare.

The next set of symptoms to be noticed relate to the condition of the skin. *Petechiæ* were almost universal. They seldom appeared before the sixth day, were sometimes very thickly distributed over the whole body, were never critical, and were very rarely observed in other acute diseases besides typhoid fever. In one instance they were brought out in great numbers by the warm-bath. *Sudamina* were very common, sometimes very abundant, never critical, and not connected with perspiration. They never appeared before the twelfth day, and were equally frequent in fatal, severe, and slight cases *Erysipelas* occasionally occurred, and never in slight cases. Sometimes it was erratic, and generally advanced with great rapidity. Of other eruptions, *erythema marginatum*, *urtica-*

ria, and *herpes labialis*, were at times observed. An important circumstance, which it was necessary to keep always in view in the treatment, was a great tendency in blisters to excite inflammation of the true skin, ending in ulceration, and sometimes destruction of the skin. This tendency, as well as a tendency to the formation of eschars on the parts subjected to pressure, was remarked chiefly in the advanced stages of slow cases.

The chief symptom referrible to the *Function of Respiration* was cough. This, however, was seldom severe. It was often attended with sonorous or mucous râle over the whole chest, in which case it was not attended with dyspnœa, and did not require the treatment of catarrh. But sometimes the râle was local and crepitating, and then dyspnœa was at times present, and hepatization was discovered in the part after death. The sputa were in general scanty, at times tinged with blood, and in a few cases puriform.

The *Blood* drawn from a vein showed in a few cases a buffy coat, but was never cupped. Much more generally it was free from buffiness, coagulated loosely, and at times formed a very loose gelatinous mass.

Symptoms of *General fever* were rarely absent. *Rigors* at the commencement were very common, often they were protracted, and frequently they recurred again and again in the subsequent course of the disease. The *Heat* of skin was often excessive, and by no means always in relation to the state of the pulse, which was not unfrequently moderate when the heat was intense. *Perspiration* was common, often recurred frequently, especially during convalescence, the progress of which it materially retarded, and in no instance was it critical. It did not bear an inverse ratio to the diarrhœa, but rather corresponded with it. The *Pulse* was at times full and not frequent, but much more generally it was small, soft, and very frequent, and in not a few instances it was fluttering, irregular, very feeble, and very hurried. In all cases of the last description death inevitably followed, and in all the heart was found softened. It is therefore not improbable that softening of the heart was a common cause of death. A rapid small pulse generally presaged a shorter attack than a full slow one. The degree of disorder in the pulse was always proportioned to the number and extent of the secondary affections.

At the conclusion of his account of the symptoms, of which a succinct view has now been given, M. Louis adds six chapters; —on the diagnosis of the disease; on cases where the affection of the intestines was latent, or not indicated by the usual symptoms; on cases where the nature of the disease was doubtful;

on cases of imitative typhoid fever; on perforation of the intestines, the effect of ulceration of the oval plates in the ilium; and on the causes of the disease.

In the first of these chapters he makes the diagnosis depend chiefly on the epistaxis, petechiæ, sudamina, flatulent distension of the abdomen, black encrusted tongue, somnolency, stupor, excessive and disproportionate debility, tendency to ulceration of blistered surfaces, and the formation of eschars, spasms of the muscles. In the next chapter he relates five cases in which the leading symptoms were wanting,—where the patients complained chiefly of languor, listlessness, thirst, some peevishness and want of appetite for twenty-three or even six-and-thirty days; were then seized with pain in the lower belly, vomiting and increased debility, and expired in a few days more. In all such cases the ilium was found perforated, and the peritonæum inflamed. Next he describes several cases where the irregularity in the course of the symptoms, the great extent of secondary organic derangement found after death, and the slight extent of the disease in the ilium rendered the nature of the disorder doubtful. He then proceeds to state the particulars of those cases where the leading symptoms of typhoid fever were present, although the conglomerate crypts of the ilium were not diseased. These were all instances of extensive softening of the mucous membrane of the small or great intestines.

The most interesting of the annexed chapters, however, is that which relates to *Perforation of the Intestines*. This subject formed the topic of an essay published by the author in 1823, and is discussed there in greater detail than in the present work. An analysis of his cases then made public will be seen in the number of this Journal for January 1824. Perforation of the small intestines is a natural consequence of the ulceration of the plates of agglomerated crypts, and is almost peculiar to that disease. Bretonneau and Trousseau never met with an instance of perforation of the intestines, where the aperture was not in the middle of a mucous gland. M. Louis met with perforation in no less than a seventh part of his fatal cases. It is indicated during life by a feeling of something giving way in the lower belly, by sudden acute pain there, extending rapidly over the whole abdomen, vomiting, excessive prostration and anxiety, and speedy death. Sometimes these symptoms are obscured by the deep state of stupor. In a few cases the early symptoms of typhoid fever are obscure or even almost entirely wanting, and the symptoms of perforation are the first indication of serious mischief. In the dead body the peritonæal surface of the intestines is found more or less extensively inflamed, and generally covered here and there with lymph. Several characteristic cases of this nature are related by Dr Abercrombie.

On the subject of the *Causes* of typhoid fever, in M. Louis' meaning of the term, the present work contains few observations. He found it most prevalent in those who had recently arrived in Paris, confined entirely to persons under forty years of age, much more frequent among males than among females. Contagion is never once mentioned throughout the whole work.

Little need be said here of the Treatment, as laid down and investigated by M. Louis. It would appear that on the whole very little advantage was derived from the treatment, for the mortality from this disease was so high as a third part of the whole,—an amount of mortality hardly equalled by any other acute disease, and certainly never approached in modern times by any of the fevers usually considered in this and other countries typhoid or adynamic fever.

He admits of venesection at the early stage of the disease; but restricts the quantity of blood which should be drawn to twelve ounces, and says that the operation can rarely be repeated with safety. Tonics are of more certain advantage; and bark or sulphate of kina were usually chosen. They were most decidedly useful in the advanced stage, and their good effects most apparent when the diarrhœa was inconsiderable, the pulse not frequent, and the debility excessive. He places little reliance on blisters; but this remedy appears never to have been used in any other way than as a revulsive or derivative on the legs. Ice applied to the head was sometimes useful.—Altogether we believe the British physician need hardly look to M. Louis' work for satisfactory information as to the method of cure in fever.

Hitherto we have allowed the author to state his views of this singular disease in abstract, without interfering in his narrative by any statement of our sentiments as to the soundness of his opinions. Before concluding, however, it is right to see what relation the facts he has published bear to the experience of physicians in the present day.

It is hardly necessary to say, that M. Louis discards the doctrine of the existence of essential fevers; and any one on perusing his work will perceive he imagines that all cases of adynamic or typhoid fever are in reality caused by induration and ulceration of the conglomerated mucous crypts of the small intestines. Such, too, appears to be the opinion of M. Bretonneau. That the local disease now mentioned occurs in a very great proportion of the cases of typhoid fever in France is sufficiently obvious from his work, as well as from the observations of prior authors, and more particularly of MM. Petit and Serres, and of M Trousseau. The last author in particular states that it is a disease as common and not less devastating than the small-pox, measles, or scarlatina;—a disease which few escape before reaching the term of their existence; which possesses, like the cutaneous febrile disorders, the singular property of affecting the same individual once only in the course of his life; and which is nothing else than the *Febris putrida genuina*, the *Synochus putris* and *imputris*, the *Mucous adynamic fever* of Pinel, the prototype of Broussais' *Gastro-enteritis*, and the *Typhus mitior* which appeared four years ago (1822) in Ireland *.

That the local disease in the intestines occurs so early in many cases as to be with justice considered the primary affection, appears well ascertained. It cannot be denied, therefore, that in France a peculiar *Entero-mesenteric* disease is a very common cause of a concatenation of symptoms which cannot be distinguished from Typhus fever in its slower form. But that all cases of typhoid or adynamic fever are of this nature may be questioned; and every British practitioner of experience in the pathology of fever will give the proposition a flat denial. In the last epidemic of this city, which ceased only a few months ago, hundreds of cases occurred of exactly the same character as to symptoms with those described by M. Louis, and very many were examined after death with great care by various physicians attached to the Infirmary and Fever Hospital; and we are sure we state the experience of all, when we mention as our own, that such enlargement and alteration of the glandular patches of the ilium as the author describes were very rare indeed, and almost always distinctly secondary in their nature. In a very few instances only has the discovery of appearances of the kind corresponded with the early developement of symptoms of intestinal disorder. It may be added, that, according to the experience of some late London physicians, this disease of the intestines has been much more frequent there than here, though still much less common than in Paris; and in not a few instances it has been difficult to say whether the fever or the intestinal disorder was the primary affection. So much has this difficulty been felt, that Dr Bright, the British author who has hitherto given the best description of the intestinal affection, has waived the discussion of the question altogether.

Among the numerous practical and pathological conclusions to be drawn from M. Louis' work, not the least interesting is the complete death-blow he gives to those theorists who maintain that every fever is a variety of inflammation of the stomach, as well as to those equally extravagant speculators who suppose it always to depend on inflammation of the brain. He shows most satisfactorily that all the characteristic symptoms of typhoid fever may be present, without either the stomach or the head being deranged except functionally.

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* Archives Gén. de Méd. x. 69.

The results of his inquiries ought to turn the attention of physicians even more pointedly than has been done hitherto to the secondary affections which occur during fever; and in particular to those singular states of softening without inflammation which are remarked in the spleen, liver, heart, and some other organs —affections which appear to be a common cause of the fever proceeding to a fatal termination, and which, if M. Louis' views of their nature be correct, are not to be counteracted by the means commonly resorted to for subduing local symptoms.

In conclusion, we must be allowed to observe, that, although his book is a copious record of most valuable and ably reported facts, we suspect few will have courage to peruse his volumes faithfully. The details of the cases are unnecessarily tedious.

ART. III.—*An account of the mode of performing the Lateral Operation of Lithotomy, with Illustrations.* By EDWARD STANLEY, Assistant Surgeon and Lecturer on Anatomy and Physiology at Saint Bartholomew's Hospital. London, 1829. 4to, pp. 23. With seven Lithographed Engravings.

THE manner of performing the lateral operation of lithotomy has, since the time of its great reviver Cheselden, been so repeatedly and so thoroughly investigated, that little would remain, it might be imagined, to be done either in the way of explanation or improvement. The anatomical history of the parts concerned in the operation has been repeatedly explained with various degrees of accuracy and minuteness. The merits of different instruments have been repeatedly canvassed, and their comparative utility practically ascertained by different operators. To explain the direction and extent of the incisions, and the most proper mode of dividing the prostate gland and neck of the bladder, though casually noticed by various surgeons, yet by no one have more pains been taken than by Mr A. Key. Scarcely any operation indeed has been more thoroughly studied in all its bearings than this; and we believe upon none so little liable to variation in the modes of accomplishing the great object, that of cutting into the bladder, has so much been written.

The appearance of another treatise may, under those circumstances, be matter of surprise. But if our surgical readers are astonished that it is possible to say any more on the lateral operation, we assure them that the present account contains matter, which, if not altogether new, is by no means undeserving of attention. Though so short that the whole might be contained in one chapter of any elementary work, it exhibits a clear exposition of the circumstances requisite to be known in order to perform the lateral operation with dexterity and success.

After mentioning the anxiety which most persons feel the first time they perform the operation, Mr Stanley observes, " In endeavouring to be serviceable on such an occasion I have experienced the want of a simple, yet sufficiently detailed account of the mode of performing the operation, unincumbered by critical or historical matter. Such an account it has been my object to supply, and, with the hope of having increased its utility, I have added some illustrations of the parts concerned in the operation, in their healthy and diseased states." Taking the essay in connection with the object here stated, it must be allowed that Mr Stanley has accomplished very creditably the task which he undertakes.

The paper consists of three sections;—the first on the mode of performing the operation, the second on the direction and extent of the incision of the prostate gland, and the third on the instruments employed in the performance of the operation.

On the first subject discussed it might be imagined that there cannot be much room either for variation or ambiguity. The object is to make from the perinæum into the bladder an incision sufficiently large to admit the extraction of a calculus of ordinary size; and the circumscribed space and the anatomical relations of the parts bring the matter into a narrow compass indeed. However just this may be in theory, we find nevertheless that, in the actual performance of the operation, errors are not unfrequent; and these must be ascribed, if we exclude anatomical ignorance, either to misconception of the true course in which the incisions are to be made, or to erroneous ideas on the manner in which the instruments are to be made to co-operate during the performance of the operation. With the view of rectifying these mistakes Mr Stanley gives a short statement of the whole necessary steps of the operation, from the first moment the patient is placed on the table to the final extraction of the concretion. After some preliminary directions about the adjustment of the staff, the best position of the patient, and the situation of the *corpus spongiosum* in relation to the raphé of the perinæum and the ramus and tuberosity of the ischium, Mr Stanley directs the incisions to be made first through the skin, fat, and tendinous fibres, and then into the urethra, with the following conditions,

" The length of the first incision should never be much less than three inches, and in the case of an unusually large stone it must be extended towards the *tuber ischii* even to four inches. Its exact line should be one-third from the tuberosity and ramus of the ischium, and two-thirds from the anus. Any deviation from this had better be by an approximation of the incision to the tuberosity than to the anus.

" In a thin person a scalpel may be plunged through the integuments directly into the membranous portion of the urethra; but with a considerable depth of fat in the perinæum this is difficult, and in attempting it the bulb will be almost certainly divided, its artery wounded, and the urethra penetrated anterior to its membranous portion.

" With the left fore-finger in the wound, the incisions are to be continued to the membranous portion of the urethra. These incisions being made in the triangular space between the *corpus spongiosum* and the *crus penis* will divide the *transversalis perinæi* muscle, branches of the perinæal artery, a small part of the triangular ligament, and the front edge of the *levator ani* muscle.

" The left fore-finger and the scalpel moving together towards the membranous portion of the urethra, and the staff being felt with the edge of the scalpel turned upwards, a slit on the urethra is then to be made to the extent of half an inch at least. The nail of the fore-finger passing by the side of the scalpel into the groove of the staff is to remain there as a guide to the beak of the knife or gorget, whichever instrument may be used to complete the operation."

These directions are so explicit, that no one who reads them can fail to understand what parts are to be divided, and in what manner they are to be divided. That the membranous part of the urethra is the point in which that canal is to be laid open, is a rule that has been established since the time of Cheselden, who takes care to mention expressly, that " he cut upon the staff, in that part of the urethra which lies beyond the *corpora cavernosa urethræ*, and in the prostate gland, cutting from below upwards to avoid wounding the gut;"—evidently meaning the membranous portion of the canal. The substance of this rule has been repeated by every author down to Camper,* Sir James Earle, † Monteggia, Boyer, Richerand, Mr Martineau, and Mr Key; and if it has been neglected, it cannot be said that it was because it was not established. The reason of the rule is obvious. Not only are the parts thus divided less complicated in structure than if the incision were more anterior, but there is less chance of troublesome hemorrhage from the vessels of the bulbous portion. Yet obvious as this must be to those acquainted with the anatomical relations of the parts, the manner in which Mr Stanley thinks it necessary to impress this rule, shows that surgeons are not at all times so attentive to it as we had imagined, from the universality of the rule, they ought to be.

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* Demonstrationes Anatomico-Pathologicæ.

† Practical Observations, &c. With an Appendix, p. 16. London, 1803.

"That there may be a certainty of opening the urethra in its membranous portion, let it be recollected, that the scalpel must enter the staff directly under the symphysis pubis: of the importance of this rule there can be no doubt. Mr Blizard, the dexterity of whose numerous operations at the London Hospital has been acknowledged in the strongest terms by all who witnessed them, attributes his success to the rule of invariably penetrating the urethra in its membranous portion. Mr Martineau, who has recorded the result of his great experience, states, 'I introduce the point of the knife into it (the groove) as low down as I can, and cut the membranous part of the urethra.' * The nearer to the prostate gland the urethra is opened the better, as it lessens the risk of wounding the bulb or its artery."

To demonstrate, indeed, the propriety of this rule, Mr Stanley might have adduced the authority, not only of Mr Martineau and Mr Blizard, but of all the surgeons who have ever operated or given directions for operating; and to us it appears singular that Mr Stanley should have occasion to impress so particularly a rule so obvious in principle, and which we understood to be universally admitted.

After reminding the surgeon that he must not confound the escape of urine, which occasionally takes place on opening the urethra, with the gush which happens on dividing the neck of the bladder, Mr Stanley proceeds to inculcate the necessity of ascertaining that the beak of the knife is adjusted to the groove of the staff, and in actual contact with it, and while the left hand is employed in retaining the latter firmly, he recommends the former be carried forward by the right, till the division of the prostate and neck of the bladder is indicated by the gush of urine. He also adds, what is very often omitted in directions regarding this part of the operation, that, in withdrawing the knife from the bladder, the incision of the prostate should be enlarged, by directing the blade outwards and downwards, so as to correspond in some degree with the external incision. This observation is so much the more valuable, that it not unfrequently happens that students, after hearing long and ela-

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* Medico-Chirurg. Transact. Vol. xi.

borate demonstrations on the mode of performing the lateral operation, are at a loss to understand whether the division effected by the mere entrance of a cutting instrument into the bladder is large enough to admit the extraction of a stone. It is indeed true, that in operating by the gorget no room for any doubt of this description is left; for it is one of the several disadvantages of that instrument, that though it may cut as it enters, it is more likely to tear and mangle as it is withdrawn, and, at all events, cannot be used for enlarging the incision laterally and downwards. It is then to the knife of Mr Blizard, and the beaked scalpel of Sir Astley Cooper, that the direction now given is applicable; and the employment of it is by no means altogether a matter of indifference.

In the first place, it must be remembered, that the moment the neck of the bladder and prostate gland are divided, the urine escapes, and the organ contracts suddenly, and in general forcibly. It requires no reasoning to show that this contraction changes to a certain extent the direction of the incision made by the entrance of the instrument, by drawing its lower angle upwards. It is therefore a matter of some uncertainty, whether, in withdrawing the knife, the operation shall prolong the original incision, or may happen to make a new one,—a circumstance which we believe has actually happened. It may be said that this is a matter of no great moment; but it cannot be forgotten that two divisions of the neck of the bladder must be more difficult to heal than one. In ascertaining experimentally the effect of such a mode of incision on the dead body, it is clear that no information whatever can be obtained; for here is wanting the contraction of the living bladder, the cause of the slight change in the direction of the incision. It is right, nevertheless, to mention, that Camper, who was aware of the inconvenience alluded to, ascribes it to the great resistance of the prostate gland, and the convexity of the catheter entering the wound.

Secondly, the most common error of beginning lithotomists, is to make the incision of the neck of the bladder and prostate too small; and this we believe is almost invariably to be ascribed to the circumstance of either using the gorget, or of simply carrying the knife forward into the bladder, and failing to give the knife a lateral and downward motion, so as to divide the left lobe of the prostate freely and extensively. In the case of the gorget, nothing more is divided than the breadth of the instrument allows. In the case of the knife, unless the lateral motion is attended to, still less is divided, since the blade is narrower; and the only alternative, therefore, is to enlarge the incision as the knife is withdrawn, as recommended by Mr Stanley. When, however, the objections to this mode of cutting now alluded to, are considered, we are convinced that it is much safer, and not more difficult, to make the division of the prostate of the proper size at first. In this there is certainly some little difficulty to the beginning lithotomist, more especially if he has dexterity to acquire; and it must not be denied, that in certain cases the extraordinary size of the concretion constitutes a cause of difficulty, which cannot be foreseen or guarded against by the most experienced. That the thing is practicable, nevertheless, we know from the testimony of several eminent operators, who have found that it was always possible to make, by the first incision of the neck of the bladder and prostate, a wound quite large enough to allow the extraction of a calculus of ordinary size.

While, therefore, we are happy to find Mr Stanley notices this mode of cutting when withdrawing the knife, we think, that, in the majority of cases, it is much better to make the incision of the adequate average size as the knife enters. This would supersede the necessity of the direction which he subsequently gives to hold the knife lightly, "that the operator, sensible of the resistance he meets, may judge accurately of its progress through the prostate," and also that of inclining the handle "towards the left ischium, but not towards the right, as the blade would then be directed towards the pudendal (the internal pudic) artery." From the anatomical relations, indeed, of the prostate to the ramus of the ischium, and the peculiar situation of this artery, its division is a much more likely occurrence on the withdrawing incision than during the introduction of the knife.

These observations apply chiefly to the use of the knife. Mr Stanley, however, admits the employment of the gorget, which he thinks by the width of its blade is perfectly adequate to divide the prostate sufficiently on its introduction. On the propriety of employing the ordinary scalpel for the division of the neck of the bladder and the prostate, he entertains some doubt, since he argues, it is not likely that this instrument can be conducted along the groove of the staff with the same precision with which a buttoned instrument which slides along it can be carried into the bladder. In this there is some truth; but it must be observed, that great allowance must be made for individual dexterity and knowledge of the parts; for we know well, that, with these two conditions, the operation may be performed by any knife almost, however simple, if it cuts; while without them all instrumental contrivances are pretty much upon a level as to utility.

The observations on preserving the adaptation, or rather what we have above termed the co-operation of the staff and knife, show how desirous Mr Stanley is to impress this rule.

"The foregoing directions imply, that, whether a knife or a gorget be used for the incision of the prostate, the operator taking the staff in his left hand, is not to change its position until he withdraws it from the bladder. But there are other methods. The handle of the staff may be depressed, in order that, by the elevation of its other extremity, any risk of wounding the bladder may be avoided; and for the position of the staff, a corresponding depression of the handle of the knife or gorget will be required to secure the continued adaptation of the two instruments. Or the staff may be left in the hand of the assistant until the incision of the prostate is completed. The operator can then pass his left forefinger along the side of the knife, while he is dividing the prostate, for the purpose of ascertaining when the incision of the gland is sufficient to permit the easy passage of the finger into the bladder.

"For the reason that the depression of the staff and the adaptation of the beak of the knife or gorget to its groove require a consent of action between the two hands of the operator, which experience only can give him, it may seem preferable to leave the staff in the hand of the assistant; but as a general rule, it is better that the operator should depend entirely on himself for holding the staff with the requisite steadiness."

Now, upon this point we must observe, that though there is no doubt as to the necessity of the continued adaptation of the beak of the knife and the groove of the staff to the successful performance of the incision, there is nevertheless great doubt as to the propriety of effecting it by the means recommended by Mr Stanley, namely by holding the staff in the left hand, while the right is employed in guiding the knife. Mr Stanley will, for instance, find that Cheselden, whom he admits to be a great model, did not hold the staff with the left hand; for we are informed, in the first place, by Dr Douglas, that "he thrusts the forefinger of his left hand into the middle of the wound, in order to press the *intestinum* rectum to one side, that it may be in less danger of being cut; and, taking a crooked knife in his other hand, with the edge on the concave side, he thrusts the point of it through the wound close by his finger, (*i. e.* the left hand finger) into the bladder;" and Cheselden himself, in his account of the second or improved mode of operating, expresses himself in terms equally distinct:—"I then feel for the staff, and cut upon it the length of the prostate gland straight into the bladder, holding down the gut all the while with one or two fingers of my left hand." * It is superfluous to inquire into the manner in which the imitators of Cheselden, viz. Ledran, Moreau, and Lecat employed their left hand; for we are positively informed, that the first placed the nail of the index finger of the left hand in the groove of the staff; and if it is not expressly stated as to the others, it results from the mode in which they operated, that they could not have used it in holding the staff. Pouteau alone held it by means of a ring, in which he introduced one finger of the left hand, while with the others he supported the scrotum,—a proceeding not approved by M. Deschamps, and apparently rarely practised by the French lithotomists of the present day. As to the English surgeons, they abandoned or forgot the method of Cheselden some years after the death of that eminent surgeon; and the gorget operation, as it was named, introduced by Sir Cæsar Hawkins, though it does not render it absolutely necessary to hold the staff in the left hand, makes it in a great measure highly expedient to do so. Sir James Earle, nevertheless, in imitation of Ledran, both practises and recommends the rule of placing the nail of the fore-finger of the left hand in the groove of the staff, to serve as a direction for the point of the knife, while the other fingers are employed " to keep open the external wound."

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* Short Historical Account in Anatomy of the Human Body, Book iv. Chap. vi.

Without adducing more authorities on this point, however, it is of very little moment by whom or by how many the practice is sanctioned. For after all, it must be by the intrinsic value of the direction as a practical rule, that it ought to be inculcated or proscribed. Mr Key is also anxious to impress the necessity of the rule; but we believe that it would be found a much safer method to follow the practice of Cheselden, and trust the staff to the hand of a judicious and attentive assistant.

The introduction of the forceps and the apprehension and extraction of the stone are by no means the easiest parts of the operation of lithotomy. On the first point the directions of Mr Stanley cannot be said to contain any thing new. Our surgical readers, however, will be curious to know what he says when the stone cannot at once be seized.

> " When the first attempt to seize the stone has failed, the forceps should be directly withdrawn, and the finger introduced into the bladder to change the situation and position of the stone. But the finger may not reach the stone. A scoop or other instrument must then be introduced, to dislodge the stone from its unfavourable situation; and it is scarcely necessary to mention the importance of introducing into the bladder but few instruments, and these as gently as possible. A stone lodged in the lower part of the bladder has in some instances been raised, and its position changed by the finger introduced into the rectum."—The latter method was used with success by Cheselden, * and is probably not only the safest, but the most likely means of bringing with-

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* Dr Douglas's Postscript to his History of the Lateral Operation. Lond. 1726.

in the blades of the forceps a concretion, which is either lodged in a sac, or otherwise placed so as to prevent it from being easily apprehended.

" But to meet," continues Mr Stanley, " the various difficulties which may occur in extracting the stone, rules will avail but little, so much must depend on the discretion and the dexterity of the operator, in selecting the method which may be suited to the circumstances of the individual case. In many instances the seizure of the stone is resisted by the forcible contraction of the bladder upon it. It has happened that a stone which, from the sensation communicated through the sound, was believed to be loose in the bladder, has been found partly lodged within a distinct cyst, or within the orifice of the ureter. Under either of these circumstances, its extraction may be difficult. There may be an adhesion of the stone to the bladder,* or a sort of hour-glass contraction of the bladder, separating its cavity into two compartments.† In most of these cases, the peculiarity in the situation of the stone is discovered in the progress of the operation. Perhaps the entrance of the gorget into the bladder may be impeded by the stone being fixed near to or within the prostate ; or on attempting to seize the stone, it may then be discovered partly within a cyst, or partly within the ureter. Some peculiarity, however, in the situation of the stone might be suspected, from the occurrence of any unusual circumstance in sounding the patient. Some years since, I operated upon a boy in whom the stone could not be always discovered, and it was doubtful whether the sound touched the stone before or after its entrance into the bladder. The membranous portion of the urethra was opened. A beaked knife was then passed slowly along the groove of the staff to the stone, which was lodged in the prostate gland."—P. 12 and 13.

These difficulties are by no means imaginary ; and, perhaps, one not the least common is that first mentioned, from the forcible involuntary contraction of the bladder itself. In operating on the dead subject, it is impossible to anticipate either the existence or the degree of this contraction ; and when the surgeon begins first to operate on the living body, he is all at once surprised by the force with which the forceps are held in the wound, and the difficulty which he experiences in apprehending the stone. Yet even this is perhaps trifling compared to the spasmodic energy with which the bladder contracts on the concretion, and resists the motions requisite to its removal. The nature of this contraction and the energy with which it is effected, are nowhere so clearly and forcibly described as in the interesting narrative of a case, given in the words of the patient himself, as recorded by Dr Marcet in the 10th volume of the Medico-Chirurgical Transanctions. In this case, in which the fact of Mr Cline being operator, is sufficient to show that neither skill, coolness, nor manual dexterity could be wanting, the patient describes his sensations in the following manner:—

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* " In admitting the possibility of the adhesion of a stone to the bladder, I must state, that I have never seen more than an irregular deposition, or a thin layer of calculous matter, which was adherent either to the mucous membrane in a diseased state, or to a soft fungous excrescence which had arisen from it."

† " Specimens of this state of the bladder are to be seen in the Hunterian Collection."

"When the forceps was introduced, the pain was again very considerable, and every movement of the instrument, in endeavouring to find the stone, increased it. Still, however, my mind was firm and confident, and, although anxious, I was yet alive to what was going on. After several ineffectual attempts to grasp the stone, I heard the operator say in the lowest whisper, 'It is a little awkward; it lies under my hand. Give me the curved forceps:' upon which he withdrew the others. Here, I think, I asked if there was any thing wrong, or something to that purport, and was re-animated by the reply conveyed in the kindest manner: 'Be patient, Sir, it will soon be over.' When the other forceps was introduced, I had again to undergo the searching for the stone, and heard Mr Cline say, 'I have got it.' I had probably by this time conceived that the worst was over; but when the necessary force was applied to withdraw the stone, the sensation was such as I cannot find words to describe. In addition to the positive pain, there was something most peculiar in the feel. *The bladder embraced the stone as firmly as the stone was itself embraced by the forceps; it seemed as if the whole organ was about to be torn out.* The duration, however, of this really trying part of the operation was short; and when the words, 'Now, Sir, it is over,' struck my ear, the ejaculation of Thank God! was uttered with a fervency and fulness of heart which can only be conceived."*

These details show that the incision into the bladder is the part of the operation neither the most difficult to the surgeon, nor the most painful to the patient; and the nature of this involuntary contraction of the bladder cannot be too forcibly impressed, since it may occur in cases of the most healthy bladders, and is in all probability the great source of difficulty and embarrassment in the majority of cases of lithotomy. It may happen, nevertheless, that the resistance to the removal of the stone takes place not in the bladder, but in the prostate, though it has been divided to the full extent; and in this case Mr Stanley properly recommends enlarging the opening by cutting the right rather than the left lobe of the gland, with the view of shunning the risk of wounding the internal pudic artery, or of cutting the coats of the bladder beyond the neck,—that is, we presume, beyond the prostate, when infiltration of urine into the extra-vesical and pelvic cellular tissue is liable to occur, and give rise to diffuse inflammation terminating in gangrene.

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* History of a Case of *Nephritis Calculosa*, *&c.* and an account of the Operation of Lithotomy, given by the patient himself. By Alexander Marcet, M. D. &c.

The observation with which Mr Stanley commences his second section, that " a difference of opinion can scarcely arise respecting the proper direction of the incision of the prostate," might be well founded if the gorget were entirely banished from the practice of the operation. Whatever merit that instrument may have in regulating the *extent* of the incision, we maintain, that it is either impossible to make it cut uniformly in the proper *direction*, or that it cuts almost invariably in a very wrong direction. Our conviction of this fact is founded not upon mere theory ; for we made a considerable number of trials on the dead subject with this instrument, and found that in scarcely two cases did it make similar incisions, and that the division of the left lobe was always in the wrong direction. It was this conviction which led us to point out to our surgical readers the merits of the work of Mr Key, who, amidst some inadvertencies, has nevertheless placed this part of the operation in a much clearer light than it was heretofore, and has shown practically how ill calculated the gorget is for effecting the division of the prostate. These disadvantages are so clearly stated by Mr Key, that we have only to refer to the article alluded to,* in order to satisfy our readers that the gorget can never be used with the certainty of dividing the prostate in the proper direction, in consequence chiefly of the horizontal manner in which it must cut, and the limited power which the surgeon possesses over its motions.

It is, therefore, not a little singular, that, while Mr Stanley represents the proper division of the prostate to be " outwards," that is, towards the left *ramus* of the *ischium* and " downwards," he should expect this to be done by the gorget as well as by the knife. This instrument, he will find, is justly represented by Mr Key to slice off the upper part of the left lobe, rather than to divide it by an incision downwards and outwards. The correspondence also between the direction of the internal and external incisions, which is doubtless an object of some moment in the operation, cannot be attained in this manner ; and can be secured scarcely by any other means than by an instrument, over the motions of which the surgeon possesses complete control. As to the injury of the internal pudic artery and the rectum, which Mr Stanley thinks may be most easily prevented by this outward and downward incision, we believe it will not require much reasoning to show, that, instead of preventing such accidents, it is very likely to cause one or other, or both. The most effectual way to guard against wounding the pudic artery, is to keep the hilt and upper and middle part of the knife, as remote from the *ramus* of the *ischium* as is compatible with the free lateral division of the gland; and, to prevent the rectum from being injured, next to its evacuation before operating, the best means are those employed by Cheselden, of keeping the incumbent parts down by the fingers of the left hand introduced into the wound.

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* Vol. xxiv. p. 160.

The *extent* of the incision Mr Stanley determines to be adequate if it permit the easy passage of the finger into the bladder. He thinks there is no practical good in stating the largest size of a stone that has been successfully removed through an incision of the left lobe of the prostate only, because the facility of extraction depends as much on the readiness with which the prostate and neck of the bladder yield as on the size of the incision. In the young subject this dilatability is greater than in the adult, and in the adult it is greater than in old age, when the gland is generally firm, and more or less hardened. When the stone is so large that the division of the left lobe only does not admit its removal, Mr Stanley recommends the division of the right side also, in consequence of which an extent of opening will be obtained, at least equal to double the first incision, while much greater dilatation is also practicable. When the concretion is still too large for such an opening, Mr Stanley recommends that it should be broken by means of the instrument contrived by Mr Earle.*

The extent of the incision depends much upon the instrument used. Though Mr Stanley is averse to giving an exclusive preference either to the gorget or the knife, he thinks that the former is preferable, because, while the width of the blade is sufficient to make the division large enough, its mode of action prevents it from cutting beyond definite bounds. This, however, is but an unsatisfactory reason for employing an instrument which its awkward shape prevents from being properly sharpened, and which, doubtless, is recommended more by its imagined mechanism than by the knowledge or dexterity which the operator is supposed to possess. To the narrow-bladed beaked knife of Mr Blizard, however, he gives the preference, because it enters the bladder as easily as a probe, and meets less resistance in dividing the prostate gland. We regret that our knowledge of gorget operations will not allow us to agree with Mr Stanley in thinking that this instrument, if properly constructed and skilfully guided, meets with so little resistance, or is otherwise so well calculated for dividing the neck of the bladder as to recommend its use in the operation. We believe that no construction can ever make it a good or useful instrument; and the mechanical facility with which it is supposed to be urged into the bladder, is but a poor substitute for that knowledge of the parts and manual dexterity which renders the operator confident merely in the use of a good scalpel. In short, the gorget is fundamentally, and in principle, a bad instrument, and unsusceptible of improvement, whatever changes be effected in its construction. Let any one who attempts to improve this instrument consider first what has been said regarding it by Deschamps, and he will pause ere he spends his ingenuity or his time upon an object so little deserving of attention. "This instrument," he concludes, "has been forged and reforged, and almost flattened. The obstinacy with which it is employed has prevented surgeons from perceiving that it has been converted merely into a cutting blade like that of Pouteau. To its extremity operators have added a small beak, which, placed a little laterally, renders its introduction constraining, and even ridiculous by the *tour de main*, to say no more, given from left to right. Such is the gorget of Hawkins, corrected by Desault,—a method which appears in the eyes of the author of this last observation, to combine the greatest utility and the least inconvenience."

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* Medico-Chirurgical Transactions, Vol. xi.

It must be indeed admitted that some of these objections do not apply to the form of gorget recommended by Mr Stanley, which in being straight, in order that its handle may be more amenable to the hand of the surgeon, and double-edged to divide both sides of the prostate, certainly makes a near approach to the double-edged scalpel of a large size. By constructing it also with both edges turned down, so as to form with the beak an angle of 45°; and with the beak so placed that $\frac{2}{3}$ of the knife is on the right side and $\frac{1}{3}$ on the left, the requisite obliquity in direction is given to both incisions, while the left is rendered much larger than the right. When the instrument is made to undergo changes so considerable in shape and construction as to destroy almost its personal identity, it may be safely asserted, that, instead of labouring to convert a gorget into a bad knife, it would be more reasonable to have recourse to a good knife at once.

Somewhat has been said of late years about a deep perinæum, as if it were a thing never before heard of. It depends chiefly on the quantity of fat, and perhaps in some degree on the size of the pelvis generally. Mr Stanley ascribes it also to enlargement of the prostate, or dilatation of the contiguous part of the rectum, the effect of which he thinks is to raise the prostate and bladder in the pelvis, and render them more remote from the perinæum. Whatever be the cause of this variation of depth in the parts to be divided, it is clear that the only recourse is to cut more deeply; and it can never be a matter of difficulty to any able surgeon to cut till he feels himself in the vicinity of the membranous part of the urethra. On this point, however, the reader will do well to consult the sixth plate.

Mr Stanley illustrates his work with seven lithographic engravings. Of these, the first four are employed in representing the natural relations of the parts concerned in the operation; and they do so pretty distinctly. In the third plate especially, a very useful, and, we believe, new view is exhibited of the relations of the neck of the bladder, by removing altogether the pubal bones and branches of the ischium, and exposing the anterior or pubal part of the bladder down to its neck, and its connection with the rectum. The position and course of the internal pudic is here well exhibited; and, to complete this point of anatomy, in the following plate a lateral view of the origin, course, and distribution of the same vessel, from its rise in the pelvis to its termination in the corpus cavernosum and penis is well delineated. The fifth represents a variety in the origin and course of the artery of the bulb, and the comparative size of the prostate gland at the respective ages of four and twelve years. In the sixth and seventh are delineations of morbid states of the bladder and prostate gland. These engravings, though somewhat coarse, are spirited, and convey good ideas of the objects, and render the work of Mr Stanley a very useful guide to the young lithotomist. Though we have canvassed several of the directions given by the author very freely, they are in general well founded; and, excepting his singular predilection for the gorget, we find little of which we can positively disapprove. The directions, on the contrary, show that he has studied the operation attentively.

Art. IV.—*Pathological Observations (Part II.) on continued Fever, Ague, Tic Douloureux, Measles, Small-pox, and Dropsy; illustrated by cases noted in the year* 1828, *from Hospital and Private Practice: With a preliminary Dissertation on the Institutes of Medicine; and an Appendix on the Origin, Prevention, and Cure of Organic Diseases, especially of the Viscera of the Cranium, Thorax, and Abdomen. Illustrated by Cases and Post-mortem Examinations.* By William Stoker, M.D. Senior Physician to the Fever Hospital and House of Recovery in Cork Street, Dublin, &c. &c. Dublin, Hodges and Macarthur. 1829. 8vo. Pp. 407.

Dr Stoker complains repeatedly in the work now before us, that his pathological theories formerly published by him* have not received notice from the Medical Journals of Britain; and seems to hint that his labours in the field of pathology are consequently in danger of passing through the press unappreciated by his contemporaries, and thus of being in a great measure lost to science. He now endeavours to avert these serious results of the reviewers' silence, by calling their attention to his farther investigations in the same line, and seems to consider that one great object of the present treatise is to attract the notice of the public to the former; for he openly avows in his title-page, that, having "lost one shaft," he has

> — shot his fellow forth of the same flight
> The self-same way, with more advised watch,
> To find the other forth.

A younger author might have acted differently. He would have suspected that all was not right, when his work was received every where in silence; and before adventuring a second shaft the self-same way, might have considered whether the lost one deserved the risk of another venture.

To speak more plainly, we fear Dr Stoker has not observed with sufficient care the signs of the times, otherwise he must have discovered that speculations on the proximate causes of disease are so far alien to the present method of investigation in pathology, that they will be a serious obstacle to the general reception of a work, of which they form the basis and professed object. It must be acknowledged on all hands, that the great body of pathologists, in Britain and France especially, are at present on the true road of philosophical inquiry,—amassing and generalizing facts; and the fruits of this system of inquiry have been so important and so obvious, that those who are guided by it will not be turned aside in search of mere speculation, even by authors of the long practical experience and high professional standing of Dr Stoker.

It is not our intention, however, again to cause the author vexation by passing him by in neglect, more particularly as his works undoubtedly contain interesting practical information, and his theoretical disquisitions are calculated to correct an important defect in the late researches in the department of pathology, by turning the physician's regard to the diseased states of the fluids, as well as to the organic changes produced in the solids of the body. Dr Stoker conceives, that the attention of the morbid anatomist has of late been turned too exclusively to the discovery of the causes and effects of disease as manifested in the solids, and that the medical world is becoming more and more a sect of pure solidists;—while nevertheless, granting, as he does unequivocally, the importance of the discoveries thus made, the origin and effects of diseased actions may be very often seen as remarkably in altered states of the blood, which are at present almost entirely overlooked. In the praiseworthy desire which he displays of reclaiming the pathologists of the day from their exclusive course we concur most heartily. It would seem but a very ordinary stretch of common sense to perceive, that the blood, which supplies nourishment to the organs of the body, is in all probability the frequent means of inducing disease and death in them. Such indeed, we presume, has been very long a prevailing belief with all sound pathologists. And, notwithstanding the author's general charge against them of a tendency to solidism, we conceive that very many confine their views to the diseased states of the solids, not because they consider these states always the proximate cause of disease, but because, in the process of diseased action, alterations effected on the solids are the earliest phenomena which their means of inquiry will enable them to discover. If the diseased states of the fluids have been less attended to than the deranged conditions of the solids, this appears to arise from the circumstance, that the former are in general much less easily appreciated than the latter. Even the more obvious changes which the blood undergoes during disease cannot be accurately investigated without an extent of leisure which few physicians can command, and instruments of research of which few are in possession; and as to the more obscure and almost evanescent departures from the healthy state of the blood which are to be looked for (on the principles of the Humoral pathology) at or before the commencement of disease, it still remains to be seen whether we possess any trustworthy criterions by which they may be recognized. On the whole, then, it would have been more correct if the author, instead of charging pathologists generally with the error of upholding the doctrines of pure solidism, had said that the attention of pathologists has been withdrawn from the fluids to the solids by the superior facility with which the phenomena of disease in the latter may be investigated.

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* Pathological Observations (Part I.) on Dropsy, Purpura, and the Influenza of 1822 and 1823, and particularly on the morbid changes of the blood and their influence on the production and cause of these diseases, illustrated by select cases and dissections. Dublin, 1823.

Whatever justice may be in these views, we fear it is not by such efforts as those of Dr Stoker, (if we except his exhortations to study it), that Humoralism is again to be elevated to a share of its former rank in pathology. He has given in his two treatises an excursive view of Dropsy, Purpura, Continued Fever, Ague, Tic Douloureux, Measles, and Small-pox; all of which discases he seems disposed to refer to certain changes in the state of the blood as their pathological cause; yet, from beginning to end of his Pathological Observations, it is difficult to discover substantial facts, or any thing better than mere speculation, to prove that, before the commencement, or even in the early days of these diseases, changes can be recognized in the state of the blood. He likewise endeavours to show, that the *sthenic* and *asthenic* forms of these diseases depend on different states of the blood. With respect to continued fever, purpura, measles, and small-pox in particular, he states that the inflammatory forms of them are marked by sizy blood, and the asthenic, typhoid, or adynamic forms by a state of the blood, in which its coagulation takes place loosely, imperfectly, and without any buffy appearance on the surface. We should imagine that these two states of the blood have been remarked by every practitioner in at least one of the diseases now mentioned, continued fever; and, on the whole, it has appeared to us a very general rule, though we are not prepared to admit it as universally true, that, in the sthenic or inflammatory varieties of fever,—in the Synocha of Dr Cullen and likewise in Synochus when its second or asthenic stage is but slightly marked,—the blood exhibits a distinct and very often an extremely thick sizy coat; and, on the contrary, that in the true adynamic fever, the mild typhus, or complete synochus, the blood is often in the half-dissolved state described by Dr Stoker, so that when coagulated it forms a loose, semigelatinous, tremulous mass. But it is impossible to allow, without more satisfactory proof than the author has adduced, that such states of the blood are severally the causes of the opposite forms of disease in which they are met with. On the contrary, we have seen the blood in both forms of fever deviating very little, so far as the external senses could determine, from healthy blood; and this has been particularly the case when the blood was drawn at a very early period of the disease. Dr Stoker supplies no facts to prove, what is essential to the stability of his doctrine, that the altered states of the blood he describes occur soon after the commencement of the fever; and still less does he supply even a presumption that they precede the febrile symptoms. It is scarcely necessary to add, if Dr Stoker had not apparently lost sight of the circumstance, that, according to the general opinion, the altered states of the blood observed during fever are, not the cause of the febrile action, but rather its consequence, or at most its accompaniment. Although this opinion, it must be confessed, may be wrong, some other proof of its incorrectness will be required by most readers than is to be found in the works now under consideration.

The quarter, however, in which Dr Stoker departs farthest from the prevailing opinions is in his view of the pathology of dropsy. He appears to suppose that an alteration in the state of the fluids, more particularly of the blood, is the most common cause of that disease. And he has fixed on the tendency to the formation of a thick sizy coat in the blood after it is drawn from the vessels, as the state of the blood which characterizes Sthenic or Inflammatory Dropsy, or that variety in which bloodletting has been found most advantageous. It is to be inferred, that he considers this state of the blood the cause of the dropsy. Here, again, as in the instance of fever, it must be objected that no proof whatever is brought forward to show that the altered state of the blood precedes the dropsy, or is any thing else than one of the concomitant phenomena, which, along with the dropsy itself, is produced by one common cause. Taking, for example, that form of dropsy which the observations of Dr Bright have shown to be connected with diseased kidney. An altered state of the blood, such as that which Dr Stoker describes, is more often observed in such dropsies than in any other variety. But it appears highly probable, that both the alteration of the blood and the dropsy arise from the diseased state of the kidneys and the morbid condition of the urinary secretion. That the altered state of the blood at all events does not produce the dropsy is evident from the fact, that the blood is not unfrequently natural, so far as the senses can determine. It has appeared to us never to exhibit the buffy coat, unless there existed actual inflammation in some internal organ, or a strong tendency to it. Many of the cases brought forward by Dr Stoker in support of his views are evidently examples of this form of dropsy. As to the rest, it is not always easy to say what was their exact nature. If, however, they were really cases of dropsy depending, not on local diseases, but on a general inflammatory diathesis, we must say of them, as of the cases which were obviously connected with diseased kidney, that the altered state of the blood *during* the dropsy affords no presumption whatever that this altered state existed before the dropsy, and was the cause of it.

Although the author does not carry the reader along with him in his attempt to demonstrate his pathological principles, it is right to observe, that much information will be received from the perusal of his works as to the treatment of the several diseases which he has taken under consideration. There are not many points of novelty, indeed, in his treatment; but the record of his experience is not the less interesting on that account. He opposes with justice the indiscriminate use of bloodletting in continued fever, and maintains that it is not only uncalled for, but even positively injurious in the adynamic or typhoid form which has of late prevailed in Ireland. We have already, in the review of Dr Burne's work on Adynamic Fever, had occasion to state, that in the late epidemic fever of this place, as in that of London and that of Ireland, blood-letting was much less frequently called for than in former epidemics. Dr Stoker even treats very lightly the practice of topical bleeding, especially arteriotomy. Blisters he highly extols. Emetics at the beginning of the disease are also commended, and James's powder throughout its course. Wine he considers an equivocal article of the treatment, and only of certain benefit when given in cases of great exhaustion. But yest is his favourite remedy. Yest, in frequent doses of an ounce, he has in the course of thirty years' experience of its efficacy become more and more partial to, from observing its effect in regulating the bowels, removing the petechiæ, and clearing away the black crust of the tongue. In proof of the superiority of his whole practice he mentions, that, during one of his trimestral periods of service in the Fever Hospital, the mortality was only one in fifty. Without pretending to call in question the propriety of his treatment, which appears to have been every way judicious, we must caution him against drawing inferences respecting the mortality of epidemic diseases from the experience of periods so short as three months. Every physician who has been in charge of a fever hospital during the prevalence of an epidemic must have remarked that, under the same treatment, and under circumstances apparently in other respects also the same, a very high mortality often occurs for some weeks or months, and a very low mortality for weeks or months before and after. On a late occasion, when the average number of patients in the fever hospital here was between 120 and 150, the mortality for six weeks was only 1 in 63; but the average for the year was about 1 in 20.—In some cases of purpura hemorrhagica, he has, like other practitioners, found blood-letting an excellent remedy, notwithstanding the symptoms which, on a superficial view, appear to contraindicate evacuations.—In dropsy, of the kind usually termed inflammatory, he has, like many others, found blood-letting also highly beneficial. Many interesting cases to this effect are given.—In ague, he has successfully used the sulphate of quina in a great number of instances of the Tertian and Quotidian types.

His work also contains some valuable observations on the changes which appear to have taken place during the last thirty years in the epidemic constitution of diseases. On this point it is impossible to present his remarks within moderate compass. The greater prevalence of *asthenia* or *adynamia*, as manifested in all diseases during the prevalence of the last epidemic fever in Ireland, is particularly touched on. Another singular fact, which his long experience has enabled him to observe, is the unusual absence of intermittent fever, and of the remittent type in other diseases for the last twenty-five years, and the tendency which appears to have been very lately exhibited to a return of the epidemic intermittent constitution, which he remembers to have been prevalent in the early years of his professional life.

We shall conclude this short sketch with an account of some pointed experiments on the coagulation of sizy blood. Doubts are at present entertained as to the cause of the buffy or sizy coat of inflammatory blood. The general idea has been, that it arises from the blood coagulating more slowly than usual, and so allowing the red particles to fall down before the fibrin consolidates. This notion has been kept up by the statements of Scudamore,* and also in some degree by those of Dr J. Davy.† But it has been doubted by many; and it is now more than ten years since we were completely satisfied that the notion was erroneous, for we repeatedly found by experiment, that a strong and thick buffy coat was formed, when coagulation began in so short a time as two or three minutes. The experiments of Dr Stoker are equally decisive. He has given twenty-seven experiments, in fifteen of which the blood was buffy, and in twelve not. Of the cases where the buffy coat did not appear, there are three where coagulation did not begin till twenty or forty minutes after the blood was drawn; and in four others it did not begin for eight minutes. On the other hand, of the fifteen cases where a sizy coat was formed, there was not one where the coagulation was retarded longer than fourteen minutes; and in all but three, it commenced not later than five minutes. Early coagulation, therefore, would appear to be on the whole more favourable to the formation of the buffy coat than tardy coagulation. But in fact we believe this connexion is accidental; and it is probable that the concurrence of rapid coagulation and buffiness of the crassamentum are not related to one another as cause and effect; for of the instances in which the blood was not sizy, there were three where the coagulation was not retarded beyond five minutes, and in one it commenced in two minutes only. Some other cause must therefore be looked for, as an explanation of this familiar phenomenon.

We ought also to take notice of another physiological inquiry of the author, because a doctrine parallel with that at which he arrives has been lately upheld by several physiologists. He conceives that one of the great purposes of the liver is to contribute to the decarbonization of the blood.

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* Essay on the Blood, 1824. † Edin. Med. and Surg. Journal, xxix. 245.

Our readers will recollect, that strong presumptive evidence in favour of this theory was given in our review of the elaborate researches of Tiedemann and Gmelin on Digestion. At present we merely allude to it, to show that Dr Stoker had formed a similar conception. It is unnecessary to enter fully into the exposition given of his opinion, because it rests on no other substantial and direct fact, except a single experiment on the rabbit, by which he thinks he found, that the blood which leaves the liver is more florid than the blood which enters it.

ART. V.—1. *A General view of the present state of Lunatics and Lunatic Asylums in Great Britain and Ireland, and in some other kingdoms.* By SIR ANDREW HALLIDAY, M. D. and K. H., &c. Underwoods, London. Pp. 101. 8vo. 1828.

2. *A Letter to Lord Robert Seymour, with a Report of the number of Lunatics and Idiots in England and Wales.* By SIR ANDREW HALLIDAY, K. H. and M. D. Underwoods, London. Pp. 88, with Tables. 1829.

THE author of these pamphlets has distinguished himself for many years by his activity in pushing forward and checking the parliamentary returns as to the state of the insane in the British empire; and we believe that the medical profession and the country at large stand indebted to him for many useful suggestions respecting the parish returns, as well as for the zeal with which, by his writings and remonstrances, he has fixed the attention of the legislature on the condition of the insane, and the necessity of its receiving amelioration.

It is quite impossible in the brief space which we can allot to the subject, that any adequate idea can be communicated of the contents of the essays whose titles precede this article; for they consist of a condensed view of numerous voluminous returns, which the author has collated and combined with indefatigable labour; and his views are already so compressed, that it is scarcely possible to present them in a more concise form.

The former of the two works is an exposition of the accommodation provided for the insane by public munificence or private generosity in the three divisions of the empire. In this work he shows the lamentable deficiency of provision for the insane population, and compares the deficiency with the ample means provided for their maintenance and treatment in some foreign countries. His statements are professedly popular; and as such will be understood by every one, and carry conviction in every mind of the propriety of farther legislative interference.

The latter work is a report of the number of the insane in England and Wales, specifying the proportion of lunatics and idiots in each county, the proportion of males to females, the number of the population male and female, the proportion of the population employed in agriculture and in manufactures, and the number of the insane who are at large, and in confinement either in public or private asylums. For the specialties of this valuable report, we must refer to the "Letter" itself. Our attention will be confined to the general conclusions which Sir A. Halliday has drawn from a collation of the returns.

1. The first inference, which cannot be denied, is *the extraordinary prevalence of insanity* in the British dominions. Supplying the defective returns by conjectural numbers, which are certainly not beyond the truth, Sir Andrew makes out, that the insane population of England and Wales amounts to above 16,000, of which nearly 14,000 are actually on the return lists. The latter number is a proportion of somewhat more than one in every thousand of the population.

2. The next inference is, that *insanity has increased with rapid strides during the last twenty years.* According to Sir Andrew Halliday's calculation, the number is now, at least three times greater than in the earlier period. This is an extraordinary fact, if true, and a most appalling prospect certainly for our posterity. But there is every reason to suppose that the increase has been greatly exaggerated; and, indeed, the inquiry is surrounded by so many sources of fallacy, that it is scarcely possible to attach any importance to the conclusion at which he arrives. There is no doubt that the returns now comprehend a far larger proportion of the actual cases than formerly; of which, indeed, Sir Andrew has himself, by means of his personal investigations, supplied convincing evidence. The great apparent source of the accuracy of the late returns, is a greater activity on the part of the individuals throughout the country to whom the duty of drawing them up was entrusted. But another circumstance to which we suspect the author has not allowed its full weight, is the greatly diminished propensity to concealment on the part of the friends of the insane. Sir Andrew need not be told, that, twenty or five-and-twenty years ago, a lunatic, or idiot, was usually hid from society as a disgrace to his family, and his whole history and existence wrapped up as far as possible in secrecy; and that not the least of the beneficial results of the constant public investigation on the subject of insanity, has been the abatement

or removal of that strange perversion of the judgment of society, on account of which diseases of the mind too often rendered the individuals who suffered under them the object of neglect and aversion. The propensity to concealment is now so far removed, that we are certain this circumstance alone will swell the number on the lists at present, compared with those procured twenty years ago, to a very large amount. This source of fallacy vitiates even the principal fact on which Sir Andrew Halliday depends for the validity of his conclusion. He states, that, in the winter of 1806–7, he made the most minute inquiries as to the number of insane in the counties of Norfolk and Suffolk; and that he is certain he "obtained as accurate an account of the number of lunatics and idiots, as it was possible under any circumstances to obtain." The number in both counties was 226; while in the Parliamentary returns of last year the number for both is not less than 638, or nearly three times as great. We cannot imagine, however, that in 1806 it was possible for any man even to approach to the true account of the insane population, in face of the obstacles which the desire of concealment would then throw in his way. Still the difference is so enormous, that Sir Andrew must certainly be held to have made out a *prima facie* proposition, to which, in the investigations to be subsequently made, the attention of the inquirer will be seriously turned.

3. The third result at which the author has arrived is, that *in agricultural districts insanity is much more prevalent than in manufacturing districts.* This proposition, too, we must venture to question. In twelve English counties, the majority of the inhabitants of which is employed in agriculture, namely, Bedford, Berks, Bucks, Cambridge, Hereford, Lincoln, Norfolk, Northampton, Oxford, Rutland, Suffolk, and Wilts, the population is a little more than two millions; and the insane, according to Sir Andrew, 2526, or one in 820. And in twelve counties, the majority of whose inhabitants are not employed in agriculture, namely, Cornwall, Cheshire, Derby, Durham, Gloucester, Lancaster, Northumberland, Nottingham, Stafford, Somerset, Warwick, and the West Riding of Yorkshire, the population is about four millions and a half; and the insane 3910, or very nearly one to 1200. Hence the relative proportion of the insane in a given number of inhabitants in agricultural and manufacturing districts is nearly as three to two. Such is Sir Andrew's statement. The result, however, is vitiated, we apprehend, in a material degree by the greater difficulty of procuring an account of the state of insanity among a manufacturing population. There is little doubt that, in the agricultural districts of Scotland, (and those of

England must be in similar circumstances,) there are more persons than one in every parish who could tell at once and exactly how many insane people reside in his parish; and at all events, we know that, with very few exceptions, this information could be at once furnished by each clergyman. But in manufacturing districts and great towns, we are quite sure that such information must be very defective, as no individual can possess the knowledge.

Taking it, however, for the present as granted, that Sir Andrew Halliday's proposition respecting the superior frequency of insanity in agricultural districts is consonant with fact, it becomes, on his part, a very desirable object to account for the difference. This he has accordingly attempted, but not, in our opinion, with success. He states, that the progress of civilization, and the diffusion of general knowledge among the lower ranks, has a tendency to increase the prevalence of insanity. Such is generally conceived to be the case, and there is every reason to suppose the belief well-founded. But it is very questionable whether Sir Andrew Halliday is correct in applying this circumstance to the explanation of the frequency of insanity in agricultural districts; for the diffusion of knowledge and the refinement of habits are generally conceived to be greatest, not certainly in the agricultural, but in the manufacturing part of the population. The general fact at which the author has arrived is therefore in contradiction with what *a priori* would have been expected. Another cause to which he ascribes the apparent frequency of insanity among agriculturists, is the severity and certain peculiarities of their labour. "I can easily conceive," says he, "how in the agricultural districts, where the people labour hard and fare ill, and where females are employed in labour that leads to violent exertion and distortions of the body, the growth of the brain may be affected, its developement impeded, and even the form of the cranium altered. We know that females will continue to labour throughout the whole of their pregnancy; and no one who has ever lived in the country will hesitate to pronounce the labour of a farm-servant most prejudicial to the growth of the fœtus in utero. The position of the body as in reaping; the constant and often violent exertions of the muscles; and even in walking in a hilly country, the changes in the position of the body, and in the compression or relaxation of the abdominal muscles,—are sufficient of themselves to affect the contents of the pelvis, and particularly in the latter stages of utero-gestation."—(P. 73.) We must confess we do not perceive the force of these statements. No fact in medical statistics rests on a firmer basis, than that the human form attains its state of greatest perfection, as to stature and strength, in the agricultural districts of a country; which is surely not very consistent with the notion that the mode of life during pregnancy, "is prejudicial to the growth of the fœtus in utero."

4. The next general fact which is deducible from the documents in the present work is, that in agricultural districts the proportion of idiots to lunatics is much greater than in districts where the population is chiefly engaged in manufactures. Thus, in the twelve agricultural counties formerly mentioned, the idiots are 1502 and the lunatics 1024, or in the proportion of *three to two* nearly; while in the twelve manufacturing counties, the idiots are 1909 and the lunatics 2001, or *nearly equal in number*. Sir Andrew might have established this proposition more clearly still, by comparing together the counties which are most purely agricultural with those which are most purely manufacturing, and thus establishing a progressive alteration of the ratio, according as the ratio of the two descriptions of population changes. Thus at one end of the scale we find that in the counties of Bucks, Cambridge, Northampton, Oxford, and Suffolk, which contain a population of 670,000, of whom three-fifths at least are employed in agriculture, the number of idiots is 846, and of lunatics only 212, that is, the idiots form *eighty per cent.* of the total insane. Taking next a set of counties still agricultural, but less purely so, we find that in Berks, Lincoln, and Norfolk, containing a population of 760,000, of whom somewhat more than a half are engaged in agriculture, the idiots are 408 and the lunatics 284, so that the idiots here form only *fifty-nine per cent.* of the total insane. Thirdly, in the manufacturing counties of Cornwall, Cheshire, Gloucester, and Nottingham, containing a population of rather more than a million, of whom one-third only are agriculturists, there are 376 idiots and 350 lunatics, the proportion of idiots being therefore fifty-two per cent. of the total insane. Fourthly, in Staffordshire and the West Riding of Yorkshire, where out of a population of 1,100,000 one-fifth only are agricultural, the idiots are 416, and the lunatics 493; consequently the former amount to *forty-six per cent.* of the total insane. And, lastly, in Lancashire, where nine-tenths of a population exceeding a million are engaged in manufactures, the idiots are 242 and the lunatics 562, the idiots being thus only *thirty per cent.* of all the insane. These differences are so great, and are derived from so large a field of observation, that they establish the general rule already laid down, notwithstanding a few exceptions which will at once appear on inspecting the author's tables. Thus in Wiltshire, where more than a half of the population are agriculturists, the per centage of idiots among the insane is not

fifty-nine, but only *twenty per cent.* and on the other hand in Warwickshire and Derbyshire, where the agricultural population is not above a third of the whole, the proportion of idiots among the insane is not fifty-two per cent. but so high as *seventy per cent.* for the former, and *eighty-four per cent.* for the latter county. These deviations, however, are not sufficient to weaken the general rule, and must be explained by the operation of some other circumstance. This explanation is afforded in the case of Derbyshire by the fact which will now be briefly alluded to,—that the proportion of idiots among the insane is greater in mountainous than in level countries. But we are not prepared to say what is the cause of the departure from the general rule in the instance of the counties of Warwick and Wilts.

5. On considering carefully the numbers of the lunatics and idiots in the five several classes of counties mentioned above, another important general fact will be discovered, which Sir Andrew Halliday has not noticed. In the first class of purely agricultural counties the proportion of lunatics to the population is one in about 3160. In the two next classes, where the inhabitants are less purely agricultural, the proportions are one in 2700 and 2850. In the fourth class, where only a fifth of the population is agricultural, the proportion of lunatics is one to 2230. And in the last class, consisting of a purely manufacturing population, the proportion is one in 1780. These differences would appear much greater, if the returns could be procured in as complete a state from the manufacturing towns as from the country parishes. Hence, whatever may be the state of the fact as to the relative prevalence of insanity generally in the two descriptions of population, *lunacy obviously is much more prevalent in manufacturing than in agricultural districts.* This is easily understood; for the greater frequency of extreme vicissitudes of fortune subjects the mind to more severe trials, which also by education and habits it is less capable of withstanding. The rule now laid down, however, places in a still stronger light the greater prevalence of idiocy in agricultural than in manufacturing districts; a fact of which no doubt can be entertained, although the cause of it is by no means evident.

6. The next general fact is that *in mountainous counties the proportion of idiots among the insane is greater than in level counties.* We have just seen an example of this in the instance of Derbyshire. The following are also interesting examples. The Welsh counties contain a population of above 717,000, of whom about three-fifths are engaged in agriculture. Here the total insane amount to 896, of whom 774 or *eighty-four per cent.* are idiots. Shropshire in a population of 200,000,

of whom hardly a half are agriculturists, has 143 insane persons in the returns, of whom 123 or *eighty-six per cent.* are idiots; and Cumberland, where about a third only of a population of 156,000 are agriculturists, and where consequently the proportion of idiots and lunatics, according to the general law last mentioned, ought to be about equal, the total insane are ninety-two, of whom eighty-three are idiots, or the large proportion of *ninety-one per cent.*

These are not the only points of comparison in which the county returns of the insane may be viewed. But there are not any other particulars in which a contrast can be distinctly established. Sir Andrew Halliday has compared together the chief maritime with the principal inland counties; but there does not appear to be any material difference between them as to the proportion of the insane generally, or of the idiots to the lunatics.

ART. VI.—1. *Sulla Storia De' Mali Venerei Lettere.* De DOMENICO THIENE, Medico en Vicenza. Venezia, 1823. 8vo. Pp. 302. *Letters on the History of Venereal Complaints.* By DOMINICO THIENE, Physician in Vicentia. Venice, 1823.

2. *Bemerkungen über die Geschichte und Behandlung der Venerischen Krankheiten.* VON. D. V. A. HUBER. Stuttgart und Tubingen, 1825. 8vo. Pp. 124. *Observations on the History and Treatment of Venereal Complaints.* By Dr V. A. HUBER. Stuttgart und Tubingen, 1825.

3. *Ueber die Lustseuche und ihre Heilung ohne Quicksilber.* VON. G. F. HANDSHUCH, &c. Wurzburg, 1826. *On Syphilis and its Treatment without Quicksilver.* By R. FREDERIC HANDSHUCH, Doctor of Medicine, Surgery, and Midwifery, Regimental Physician of the Royal Bavarian Artillery, &c. Wurzburg, 1826.

4. *A Treatise on Syphilis; in which the History, Symptoms, and Method of Treating every Form of that Disease, are fully considered.* By JOHN BACOT, Surgeon to the St George's and St James's Dispensary, and lately Surgeon to the Grenadier Regiment of Foot Guards. London, 1829. 8vo. Pp. 280.

5. *A Practical Treatise on the Diseases of the Genitals of the Male, with a preliminary Essay on the History, Na-*

ture, and General Treatment of Lues Venerea. By JOHN MADDOX TITLEY, M. D. London, 1829. 8vo. Pp. 403.

IT is a proof of the inadequacy of historical evidence to determine ambiguous and minute points of scientific research, that, though syphilis has been known to European physicians for at least three centuries, the question as to its modern and exotic origin, or its existence from remote antiquity, is not yet decided. This, indeed, cannot altogether be ascribed to want of investigation, for the subject attracted attention at an early period; and though the majority of medical authors quickly adopted the idea that it was a new disease, unheard of and unknown, doubts appear to have been entertained on the accuracy of this opinion; and that these doubts were not altogether without foundation, is shown by the discordant hypotheses which those who admitted its new existence maintained. Thus, though Grundbeck, Alexander Benedict, Leonicenus, Montagnana, Caspar Torrella, De Vigo, Alphonso Ferri, Fracastorius, and others, are unanimous in regarding syphilis as a disease entirely new, we find that Gilinus, Torella, Hock de Brachenau, Phrisius, and the noted Ulrich de Hutten, give it an astrological origin; whereas Leonicenus and Montesaurus ascribed it to humidity of the air, in consequence of inundations of the Tiber, Rhine, and Po; Maynard to the intercourse of a common woman with a leprous knight; Brassavola to that of numerous soldiers with a woman affected by a malignant ulcer of the uterus; Fallopius to the Spanish soldiers poisoning the wells, and bribing the Italian bakers to adulterate the bread with lime; and Caesalpinus to the use of wine tainted with the blood of leprous patients. Vigo alone, who gives the most distinct history of the appearances and characters of the disorder, states simply, that it appeared throughout all Italy in the month of December in 1493, "when Charles the French King took his journey into the parts of Italie to recover the kingdom of Naples;" but though he admits it to be contagious, he takes no notice of any cause from which it was alleged to arise. Vigo, however, was aware of the passages in Celsus and Suetonius, which have since been so often adduced to prove the antiquity of syphilis.

These extravagant and opposite opinions, which were well refuted by Astruc, soon gave place to one which historical coincidence, and the talents of the writers by whom it was espoused, rendered almost universal not only among physicians, but writers in general. The discovery of Hispaniola, which was accomplished in December 1492, the return of Bartholomew Columbus in the end of 1494, with Pedro de Margarit, a

Catalonian gentleman, labouring under the symptoms of the venereal disease, and the return of Christopher Columbus in June 1496, with two hundred soldiers reported to be diseased in the same manner, were conceived to be facts sufficiently strong to justify the inference, that syphilis was imported from the newly discovered island. The testimony of Oviedo, Guicciardini, Lopez de Gomara, and other writers, either contemporaneous with, or soon after these times, were allowed to establish the same point; and the American origin of syphilis, if not directly demonstrated, was at least very universally believed.

The first authors by whom the American origin of syphilis was formerly denied were Marcellus Cumanus (1495,) and Sebastian Aquilanus (1497,) the former of whom considered it as identical with a cutaneous disorder of Campania, described in the 13th century by Papia of Lombardy, and evidently a variety of leprosy. Not long after, the same opinion was maintained by Symphorian Camperio (1517,) John Lange (1533,) Michael Angelo Biondi (1542,) and Rembert Dodonæus.[*] In 1559 Francis Vallesius, a Spanish physician, maintained that it was known to the ancients, and referred to this cause the emaciation, baldness, and ulcerated countenance which Tacitus ascribes to the Emperor Tiberius.[†] In the beginning of the 17th century similar opinions were entertained by Zacutus, the Portuguese Jew, and towards its conclusion by Maynwaring (1682,) and Musitanus (1698,) the last of whom quoted the authority of Celsus, Galen, Martial, and even Avicenna, to prove the ancient existence of the disorder. Most of these authors, however, were chiefly distinguished by a feeble dissent from the received opinions; and none of them appears to have adduced facts or arguments sufficiently conclusive to make any permanent impression on the minds of physicians.

The first author who seriously undertook this task was William Beckett of London, who, in 1718, adduced from documents of the 14th century a number of curious and interesting facts, tending to show that the genitals of both sexes were liable to an affection, denominated in the language of the times the *perilous infirmity of brenning or burning*, and by which he undertakes to prove is meant the venereal disease. The manuscript from which the most authentic and pointed extract is taken, is indeed supposed to be written about the year 1390,—a full century previous to the alleged appearance of syphilis in Europe; but the disease to which it alludes corresponds more with gonorrhœa, than with what subsequent ages have shown ought to be regarded as the genuine effects of *lues*. In a subsequent number of the Transactions, Mr Beckett resumed this inquiry; and after showing that Leonicenus, Torella, and Sebastianus Aquilanus speak of it as a disease formerly known, and that, though Ulrich de Hutten maintains it was unheard of till 1493, he is nevertheless guilty of a glaring anachronism, in asserting that his father was affected with the symptoms of a disorder of which he wrote only in 1519. He then shows from Arden, from the case of John of Gaunt recorded by Gascoigne, and from the general events and belief of the times, that the existence of a disease derived from sexual intercourse, affecting first the genitals and then the whole frame, was a thing well known and generally admitted previous to the discovery of America and the Neapolitan siege. It is unnecessary to dwell particularly on his quotations from John of Gadisden or Gilbertus Anglicus, which, though evidently referable to some local affection of the organs of generation, are nevertheless too vague and inaccurate to become the foundation of positive conclusions. That from Glanville, which refers to eruptions on the face and affections of the mucous membrane of the nose and throat, is certainly more pointed; but it has the disadvantage of being equally applicable to leprosy, and some other disorders of the mouth and nose as to genuine syphilis. We shall see, nevertheless, that it is admitted as reasonably good evidence by Thiene and Huber.

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[*] The period at which these authors published their opinions on syphilis is not accurately ascertained. The birth of Camperio is placed by Sprengel at once in 1472 and in 1535. The latter is the period of his death. One edition of his works was published in 1517. That of Dodonæus is placed in 1517. But of neither is the period at which their writings appeared specified.

[†] "Quippe illi pergracilis et incurva proceritas, nudus capillo vertex, ulcerosa facies, ac plerumque medicaminibus intertincta." Annal. iv. 57.

The views of Beckett were opposed with much learning by Astruc, who, after a full examination of all the authorities for the ancient existence of syphilis, proved to the satisfaction of the majority of medical readers that the venereal disease was unknown in Europe before the discovery of Hispaniola, and that it was conveyed into Italy by several of the soldiers of the Spanish army, who had either contracted the disease in Spain after being brought from America, or who had brought it directly thither either in the first, second, or third voyages which had already been made to these remote countries. Its origin and prevalence among the natives of America, though admitted by him to be difficult of explanation, he thinks, nevertheless, may be justly traced to bad food, the effects of the tropical heat on the animal secretions, and especially to the incontinent lives which they habitually lead. It is easy to see that this, though it affords a probable, by no means gives a positive explanation of the phenomenon, and leaves the matter in as great ambiguity as ever. The learning and plausibility of Astruc, however, were received among physicians as demonstrative evidence; and we may add, that the eloquence of Robertson, the historian of America and Charles V., contributed especially among general readers to support and strengthen the cause of the modern and exotic origin of syphilitic disorders. It is almost superfluous to remark, that the latter author agrees with the majority of writers in regarding it as an established fact, that the disease was unknown before 1493, and between that and 1499 that it had spread over the whole of Europe.

In this state of matters the question was once more agitated by Don Antonio Sanchez de Ribeiro, a Spanish physician, who, in 1765, published a dissertation, in which he undertakes to prove that the disease was not of foreign origin, but took its rise in Europe as an epidemical disorder. Sanchez de Ribeiro was, however, by no means a match for Astruc. Void of his acuteness, and inferior to him in reading, he is careless in the examination of the necessary authorities. Though his language is confident, his reasons are feeble, and his conclusions are often hasty. It is observed by an author presently to be mentioned, that his whole treatise is pervaded by an erroneous principle, that syphilis was introduced by the second instead of the first voyage of Columbus. So far as the hypothesis was capable of refutation, it was partly refuted by Haller and Van Swieten, and partly consigned to silence.

The bold and confident tone of Ribeiro had nevertheless one good effect, that of exciting first the doubt, and then the research of the learned and laborious Hensler, who in his first volume on the history of syphilis, has adduced numerous authorities illustrating the history of the first appearance of this disease. Though he does not positively maintain that syphilis existed during the middle ages, he nevertheless thinks that it is intimately connected in origin and nature with the leprosy of antiquity, which he regards as the precursor in western Europe of the syphilis of the 15th century. Hensler, nevertheless, with all his learning, is somewhat partial. He reposes too much confidence in the assertions of Sanchez de Ribeiro, and is something uncandid to Astruc. He quotes few of the original passages for the American origin of syphilis; he has read none of the writings ascribed to the first discoverer of America; and he is accused by Girtanner of quoting those passages chiefly which are said to be supposititious. *

Hensler was followed by Christian Godfrey Gruner, who, however, did not altogether supply the defects, or rectify the errors of the former. He, indeed, collated the writings of the physicians who flourished from the end of the 15th to the be-

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* Geschichte der Lustseuche Von Phil. Gab. Hensler, Dr. u. s. w.

ginning of the 18th century, both in favour of and against the new character and foreign origin of the disease; but he has not examined the testimony of the historians and annalists of the time. It is therefore not wonderful that he regards the French disease as new and unknown to the ancients, since neither by name, by origin, by causes, nor by mode of treatment, is any similar disorder noticed in their writings. In short, Gruner has chiefly followed the steps of Astruc, and has done little more than exhibit the authorities originally adduced by that learned writer. He is further not very attentive to chronological order,—a point of the utmost importance to the historian, whether civil or literary. *

A new and perhaps still more able advocate for the exotic origin of syphilis appeared in Christopher Girtanner, who in a learned work on the treatment of syphilis first published in 1796, examined with particular accuracy the historical evidence regarding the circumstances under which it made its first appearance in Europe. † Girtanner adduces not only all the authorities already brought forward by Astruc, but several others either neglected or unknown by that author, derived chiefly from the civil historians and annalists of the time. By means of the former he proves the three following propositions: 1*st*, *that the venereal disease was unknown to the Greeks and the Romans*; 2*dly*, *that it was entirely unknown in Europe previous to the year* 1493; and, *thirdly*, from Leonicenus, *that it was still unknown in* 1493.

The chief defect of the evidence of Astruc is the circumstance, that his authorities are derived from the writings of the physicians and surgeons of the 14th century, who, when they saw a disease unrivalled in the rapidity of its propagation, and the severity and extent of its ravages, forthwith inferred that it was new and unheard of. The idea of ascribing it to importation from America seems to have originated with Leonard Schmaus of Salzburg, an obscure author, who wrote in 1518, and who, as he was remote at once from the place and the time at which the disease made its first appearance, cannot be expected to give very trustworthy testimony. Another defect of the evidence of Astruc is, that in no instance does any one of his authorities state positively that such a disease as syphilis was actually prevalent among the inhabitants either of Hispaniola, or any other of the newly discovered lands. The principal circumstance on which the opinion at this early period appears to have rested, is the uncertain notion that nature acts on the principle, *ubi morbus ibi remedium*, and that, as guiacum, the great remedy in these days, was brought from the newly discovered lands, so the disease for which it was an antidote was also the product of the same soil. This mistake appears, as Sprengel remarks, to have misled Guicciardini, and numerous subsequent authors,—all too remote from the period in question to examine the matter with accuracy.

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* Antiquitates Morborum Vratislaviae, 1774. Aphrodisiacus, Jenae 1788. De Morbo Gallico, Jenae 1793.

† Abhandlung ueber die Venerischen Krankheit Von Dr Christoph. Girtanner Herzsgt. Sachsen Koburg. Geheim Hofrathe; u. s. w. Gottingen, 1796 und 1802.

Under these circumstances, it is some merit in Girtanner that he recognized the necessity of obtaining the evidence of writers so near the period when the disease was first observed, that they could adduce either ocular testimony, or at least testimony so near the time and place as to remove the doubts in which the subject was certainly involved. Upon this principle, Girtanner refers to the authorities of Ferdinand Columbus, the son of Christopher, in the account of whose life he gives some account of the customs of the natives of Hispaniola, Francis Lopez de Gomara, a Spanish priest, chaplain to Cortes, the author of a general history of the Indies, Oviedo, author of the *Relacion Sommaria* and the General and Natural History of the West Indies, Peter Martyr, Hieronymus Benzoni, Herrera, Augustin de Zarate, Jacquez, Morgues, a monk, Pedro de Cieça de Leon, Guicciardini, the Spanish physician Bernard Diaz de Isla, the Italian physician Giam Battista Montani, and, lastly, the anatomist Fallopius. From these several authorities, Girtanner thinks himself justified in establishing the following propositions :—1. Syphilis was in the Antilles a common and well-known disease, with which the natives were all tainted. 2. In the continent of America syphilis was a very common and usual disease 3. It was milder in America than afterwards in Europe. 4. Before the Spaniards visited America it was entirely unknown to them. 5. By intercourse with the American females the Spaniards were infected by it. 6. The effects of the infection were so severe, that while many died, the residue were attacked by excruciating pains ; and Columbus and Pizarro lost the majority of their crews by this disorder. 7. The infected Spaniards, who had not till now seen this malady, and were ignorant of its true causes, ascribed it to the influence of the climate, the scanty food, or the consequences of the long and fatiguing voyage. 8. By the crew of Columbus syphilis was imported into Spain evidently at the first voyage, and afterwards by the subsequent ones. 9. The first place in which it appeared was Barcelona ; and 10, Sundry circumstances concurred to propagate the disease in a few years throughout all the parts of the habitable globe.

However logical the arrangement of this string of propositions may appear, the foundation on which they rest is by no means so secure as might be imagined. To avoid increasing the size of his work, Dr Girtanner does not adduce the exact passages referred to, and satisfies himself with a general reference to the Gottingen library, in which the original works are contained. When these are examined, the evidence turns out to be much less pointed and conclusive than the author represents. It is for this reason that Sprengel, who was the first to inspect the authorities adduced by Girtanner, while he allows him the merit of perceiving the necessity of consulting writers contemporaneous with the period at which syphilis first appeared, does not hesitate to express his doubts whether Girtanner actually perused or even consulted carefully all the works to which he refers. The first evidence adduced by Girtanner is at once insignificant and equivocal. It is that of Ferdinand Columbus, or rather of a Roman friar named Pane, who, in an account of the customs and usages of the Indians of Hispania, states the general prevalence of a disease named *Caracaracol*, attended with great roughness of the skin, and ascribed to supernatural agency. The second testimony, that of Oviedo, though clearer, is nevertheless liable to the same objection as that of Guicciardini, that the author proceeds on the erroneous principle of expecting to find the disease where the guiac, its remedy, is produced, and is further blameable in ascribing to the American natives the most horrible vices, in order to justify his own severities to this unhappy race. As to the Spanish physician, Roderic Diaz de Isla, Sprengel maintains that there is no proof that he was an eye-witness; and Girtanner exposes himself to the charge of want of candour in not stating the authority on which he asserts that Diaz lived in the days of Columbus. Herrera lived too late, and, it is reasonable to infer, derived his information from Oviedo; and much the same may be asserted, he maintains, of subsequent authors also quoted by Girtanner. In short, Sprengel infers, that while we are to regard the *Caracaracol* as a species of leprous tinea endemic among them, there is no proof that the genuine venereal disease, such as it appeared in Europe, had arisen among a people so simple and uncorrupted as the most impartial testimonies agree in representing the native Americans to have been.

A more positive proof still is given by Sprengel that the American expeditions of Columbus are not entitled to the merit of communicating the disease to Europe. Syphilis, according to the testimony of the most reputable authors, had already appeared in the summer of 1493, and almost simultaneously in all parts of Europe. According to Oviedo, however, unquestionably the most pointed and credible of all the witnesses on the subject, the fleet of Gonsalvo, which conveyed the Spanish soldiers to Italy, and arrived at Messina on the 26th May 1495, communicated the disease to the Italians. Throwing out of the question, therefore, the historical improbability of the fact, that the soldiers or attendants of this fleet were to have any communication with those of the hostile armament of Charles VIII. so as to infect them with syphilis, the disease was already in existence two years before, and was now spreading rapidly among the soldiers of the French army.

The weakness, indeed, of the arguments adduced by Girtanner is so obvious, that Cappel, professor of medicine at Gottingen, in the second edition of the work of the former published in 1802, in a very judicious and temperate note, expresses himself totally dissatisfied with them. The first and second propositions of Girtanner, regarding the prevalence of syphilis among the natives of the Antilles and the American continent, he regards as nothing more than conjecture. On the third and fourth, he remarks, that, from the facts now collected, it appears that the inhabitants of these countries suffered from a peculiar cutaneous disease, which was unknown to the Spaniards; but that the identity of this cutaneous affection with that which was afterwards observed in Europe as the venereal, is by no means established, and the assertion that this was mild with the aborigines, is nothing but an improbable conjecture. The circumstance of the Spaniards being tainted by the American females, and being attacked by their peculiar disease, which was probably not their only source, proves little or nothing for the assertion of Girtanner. The arguments by which he opposes the other propositions, it is unnecessary to repeat.

Opponents still more formidable to the American and modern origin of the venereal disease are found in Dominico Thiene and Dr V. A. Huber, both of whom have examined the passages of the original authors referred to by Girtanner with very great care, and nearly with the same general result. The reading, or at least the research, of both is accurate and extensive, and, while they have availed themselves with great success of the previous labours of Becket, Friend, Astruc, Hensler, Gruner, and Sprengel, they have added the strongest confirmation to the conclusions already drawn by the latter writer.

It is doubtless a proof of the carelessness as well as of the presumption of Girtanner, that he asserts that Christopher Columbus himself published nothing. Thiene finds that two letters of this Captain are extant, one written from Lisbon in the year 1493, to the minister of the treasury of Spain, the other of 1503, dated from Jamaica, and addressed to the Spanish sovereigns. In neither of these, however, is any notice taken of the existence of any disease, either under the name of Guaynaras, Hypas, Taybas, or Ycas, as it was termed, or under that of *Caracaracol*, as related by Ferdinand Columbus. The latter by the testimony of Peter Martyr, and that of the Roman monk Pane, was a species of itch or scab which was observed in some of the natives. To convert this without ceremony into syphilis, as Girtanner has done, is so poor an argument, Huber remarks, that to recognize its weakness it has only to be read. Another passage to which Huber allows greater weight, is one in which the same friar Pane speaks of some one who was affected with the French disease, (plegado del mal que llamamos Frances.) But it is manifest that this indicates nothing more than that some of the crew had carried the disease from Europe, —a point also which is clearly established by a passage in a letter of Columbus, dated 1498, relative to the state of the colony of Isabella, in which he says there might be 160 men,—affected, however, with the French disease, (160 hombres, llenos del mal Frances.)

The testimony of Oviedo, which is examined briefly by Thiene, and at greater length by Huber, may be very easily disposed of. Placing out of the question the prejudices which both Sprengel and Thiene assert disqualify Oviedo for an impartial witness on this point, it is justly remarked by Huber, that, on the supposition that the American origin of syphilis is the true one, it must have been conveyed by the crew of Columbus to Spain, and thence by the troops of Gonsalvo de Cordova to Naples. This hypothesis further requires positively that Columbus imported the disease in 1493 on returning from his first voyage; for this, Huber remarks, is the decisive point. How does it happen then, he asks, that Oviedo lays no particular stress upon this circumstance, but upon the few occasions on which he speaks of the disease, places its importation on the return from the first and the second voyages indiscriminately? It is indeed remarkable, that, when speaking of the first voyage of discovery and his return, he says not a syllable of Columbus having found in Hispaniola, and conveying to Europe, such a disease as the venereal; and even while he enumerates four remarkable events of the year 1493, he takes no notice of the occurrence of this malady, which was perhaps as remarkable as any of them. When, therefore, he proceeds so far as to adduce the instance of Pedro Margarit as one of the individuals in whose person the disease was imported, since this cavalier returned in 1496, he forgets that this was three years after the disease was known in Italy and Europe in general, and that, if Pedro Margarit really laboured under syphilis, he was more likely to have conveyed it from Europe to America than from the latter to the former country.

It is further observed by Huber, that, according to the account of Oviedo himself, the great Captain Gonsalvo de Cordova, whose troops are accused of conveying the disorder from Spain to Italy, arrived at Naples in 1495; and the disease, according to Oviedo's positive testimony, was first brought to Barcelona in 1496, where it first attacked the common people, and some months after, in the same year, spread among the higher ranks. Since it is notorious, however, that it was raging at Naples in 1494, Oviedo, he observes, by his own statement of facts, refutes his theory of the American origin of the disease, and is thus convicted of a glaring anachronism.

Upon the whole, therefore, Huber, without imputing bad faith or intentional falsification to Oviedo, thinks that it was not unnatural for him to construct a plausible enough theory of the origin of the disease from the concurring circumstances with which its appearance was attended. These circumstances were four; 1. the appearance of the disease in Barcelona in 1496; 2. the simultaneous return of Columbus and his companions, actually labouring under various forms of disease, which Oviedo in other passages sufficiently explains by the long hardships, the unusual climate, and the scanty allowance of provisions; 3. the accidental appearance of syphilis in Italy and other countries of Europe at the same time; and, lastly, its existence in the West Indies, though it came thither some years later, and perhaps the existence of what was thought to be a positive antidote. Though the attempt to connect these different circumstances, in order to give a plausible explanation of the appearance of syphilis in Europe, might easily mislead him into a chronological mistake of from two to three years, it does not excuse later researches, when the theory of Oviedo is submitted to examination as a historical fact. It appears, therefore, that the testimony of Oviedo, vitiated as it is by a manifest anachronism, amounts to nothing in proving the importation of the venereal disease from the Antilles on the American continent; and, so far as his account goes, there is an equal chance for the disease having been conveyed from Europe to America.

From the evidence now adduced, it results merely that it is not proved that syphilis was imported into Europe in the manner represented by most writers after Oviedo. It appears farther, that though Columbus and his crew came home in a sickly state, no mention of any new or unwonted disease is made either in the Chronicle of Garcia de Resende, or in that of Ruy de Pina. Both mention the prevalence of a great sickness or plague communicated to the Spaniards and Portuguese by the Jews, as was invariably asserted of that calumniated race; but no notice is taken of any disease which could be compared to the venereal, either in characters or mode of propagation. It is also justly observed by Thiene, that, if any disease of this kind had been at that time believed to be imported by Columbus, the enemies whom his discoveries created, Bovadilla, Ovando, Roldano, Fonseca, and others, would never have overlooked so excellent an opportunity of rendering him obnoxious both to the civil and the religious authorities of the country.

The authority of Peter Martyr, to whom too much importance has been attached, is really of little weight. He was not the companion of Columbus on his voyages; and although his familiar friend, of the same nation, and a member of the council of the Indies, in the first work which he wrote on the discovery of the new countries, and which was composed from the dispatches, the verbal narratives, and the letters of Columbus, and his associates, makes no mention of the venereal disease. In his epistles, speaking of a friend attacked with this complaint, he speaks of it by its ordinary denomination of *morbus Gallicus*, and never gives a hint of its American extraction. There is further some reason to believe that this letter is either fictitious or in some manner falsified. It is dated April 1488, five years at least earlier than the term *morbus Gallicus*, or the Spanish name of *Bubas* was in use; and it bears the address to *Arius Lusitanus*, the Portuguese, professor of Greek in the University of Salamanca, in which it appears Greek was not taught earlier than 1494, or rather than 1508. This proof of fabrication is nevertheless overlooked by Mr Bacot, who allows it, rather unintelligibly, to be good evidence. By Dr Titley, on the contrary, the imposture is recognized; and in rejecting its evidence he is supported by Sprengel, Thiene, and Huber. The verbal alterations are perfectly correct, as may be seen in the original recorded by Thiene.

The testimony of the Portuguese physician, Roderic Diaz de Isla is still less satisfactory. That this physician resided in Barcelona, though asserted by Girtanner, is extremely doubtful, more especially as the latter author gives no authority for the fact—a point too important to be admitted on mere assertion. Girtanner further acknowledges that he knew the work of Diaz de Isla only by the Latin translation of Welsch; and that he perused even this, Thiene shows is more than questionable, since he seems to be quite ignorant that De Isla dedicates his work to John III. of Portugal, who ascended the throne of that kingdom in 1553, and that he speaks of the *morbus Gallicus* as a disease already existing for fifteen years

—a circumstance which shows that De Isla is to be classed with the writers who treat of syphilis, not in its commencement, but after its general prevalence. These circumstances show that little reliance can be placed on the statement which De Isla gives of the processions reported to have been made in Barcelona in 1493 with the view of arresting the ravages of syphilis. Huber indeed shows from contemporaneous chronicles that during this year there was no sickness which was thought to require this mode of propitiation; and since both 1483 and 1504 are mentioned as years so distinguished, he infers that 1493, if really a period of general affliction, could not have been omitted. He further observes, that, if the disease, according to the statement of De Isla, was really prevalent in Barcelona, how does it happen that it did not spread to the contiguous provinces of the south of France, but took the direction of Naples, when a few leagues might have brought it to Montpellier, Perpignan, and Carcassone?

In this point of the investigation, however, another view is presented. Admitting, what seems to be agreed upon among the majority of authors, that syphilis was prevalent in Barcelona in 1495, is it not most likely that it appeared in this city in its progress from France? That this, whether a matter of fact or not, was a general belief not only among physicians, but among the vulgar, appears from a document which was published by Thiene, we believe, in Brera's *Giornale di Medecina* for August 1817, and of which a copy is also given in the appendix to the present work. This is a letter from Nicolaus Scyllatius, a physician of Messina, distinguished as the editor of a black letter edition of the *Rosa Anglica* of Gadisden in 1492, and addressed to Ambrosius Rosatus, first physician of the Dukes of Milan. From this it appears that Scyllatius was in Barcelona in 1494. The disease was spreading among all ranks, and was propagated according to report by contact only. It was then universally believed to have proceeded from Provence, where it was named the *disease of St Ment.* Scyllatius says he thought it the *Sahafathi* of Avicenna and the Arabians; but while he describes its general characters with great accuracy, he says nothing of American origin, but applies to Ambrose Rosatus, as skilled in the causes of disease and the influence of the stars, for information on its origin and the mode of opposing its ravages. This is the most ancient account of the disease extant, and has been overlooked by all previous authors.

The other authorities adduced by Astruc, and especially by Girtanner, are insignificant or equivocal; and from the general silence observed by the contemporaneous writers, and especially the Spanish and Portuguese chronicles, it appears that no proof exists of the doctrine so generally received, that America is the parent of this malady. From the concurring testimony, indeed, of various authors who lived at, or not long after the time, especially from that of Pietro Pintor quoted by Sanchez, of Torrella, Summaripa, Cumanus, Alexander Benedict, Julian Tani di Prato, Leonicenus, and de Vigo, it results that the epoch at which syphilis appeared in Europe was anterior somewhat to the epoch of the historical events to the occurrence of which it has been ascribed. Another inference of less moment perhaps is drawn by Thiene, that the slow operation of the venereal contagion, such as it is observed in individuals, cannot be easily reconciled with the immense number of persons infected at the same time, in countries, however remote from each other, and probably with very little mutual intercourse.

When, indeed, we consider this part of the history of syphilis, so general, so simultaneous was its appearance in many different places, and so rapidly did it manifest itself in one place after another, that it seems actually impossible to determine the place in which it was first recognized; and there are just as many and as strong arguments for its first appearance in one country as in another. Thus, though Petronius, Caspar Torrella, Haschard, Ulrich de Hutten, and Borgarutius, place its occurrence in Italy in 1493; John de Vigo, Nicolaus Massa, Cataneus, Wendelinus Hock, Schmaus, Fallopius, and the majority of authors, place it in 1494; Antonius Musa Brasavolus fixes it in 1495; and Montesaurus, Phrisius, Mainard, Benivenius, and Montanus refer it to 1496; while Fulgosi places it so early as 1492; and Fracastorius alone satisfies himself with the vague and erroneous statement, that it appeared about ten years before 1500. Some of this discordance may perhaps be explained by the *different dates* at which the disease became known in the several situations in which the authors lived. But as most of these authors wrote not as eyewitnesses of the disease, but from the testimony of others, it is easy to see that there is room in a point of this description for a good deal of variation. One point alone is certain, that, whether the appearance of the disease is fixed in 1493 or 1494, which is the most likely, it showed itself most speedily after in all the principal cities of Europe. According to the Mansfield Chronicle, the Leisnig Chronicle, the Leipsic Annals, and the Zweifalt Annals, it was general in Germany in the summer of 1493; and it is even said to have prevailed at least four years in Misnia. In the same year it was common in Auvergne; in 1494 it was known at Paris; in Augsburg it first showed itself in 1495; next year it wasknown at Memmingen, at Nurnberg, and also at Edinburgh under the name of the *Grandgore;* and in 1499 it had spread through Bohemia. It must not be denied, however, that it is remarkable, that, of all the chronicles of the cities of Spain, Barcelona, Valencia, Murcia, Toledo, Seville, Burgos, Guadalaxara, Valladolid, and Segovia, none make the slightest mention of the exact period at which the disease appeared; and the same is true of the contemporaneous and recent annals of Portugal. It may well be asked, how it happened to appear in all these places so remote from each other within periods so short, and what were the circumstances which tended to propagate the disease, if such a term may be used, in so many different directions?

We have hitherto confined our observations chiefly to the inquiries of Thiene and Huber; because, next to Sprengel, they are entitled to the merit of originality, in so far as they demonstrate the chronological errors of Oviedo, and show that the theory of the American origin of the disease is irreconcilable with the known facts attending its original developement. Nearly the same views, we now remark, are taken by Mr Bacot and Dr Titley, who, after an examination of the evidence on both sides, have come, though by different routes, to much the same conclusion.

The former author, while he justly denies the competency of the facts adduced from the Greek, Roman, and Arabian authors to prove the existence of the disease in ancient times, or even in the middle ages, further agrees with Friend in thinking, that, had the disease really existed, it could not have escaped observation, much less would it have been overlooked either by the satirical and moral writers of antiquity, or by such skilful observers of the times as Dante and Boccacio. The testimonies adduced by Mr Beckett, he justly observes, show only that gonorrhœa was not unknown as an effect of indiscriminate sexual intercourse, but prove nothing concerning *lues venerea*. Nor does he allow greater weight to the story of John of Gaunt, quoted by that author from Gascoigne. But he expresses his surprise, that the passages which are quoted by Friend from Lanfranc, Gordonius, and William de Saliceto, have escaped the notice of that strenuous advocate of antiquity; for though they prove only the existence of gonorrhœa, they might have, with the same justice as his other testimonies, been employed to demonstrate the antiquity of the venereal disease. It is unlucky, however, that Mr Bacot has been rather careless in his account of the facts and reasonings of Beckett; and not only confounds the statements of that author in his first paper with those of his second, but does not altogether do them justice. It is not in Mr Beckett's first, but in his second paper that the authority of John of Gadisden and Gilbertus Anglicus are quoted; and the extracts adduced by Beckett do not relate to the direction, that the female patient should leap backwards, which, indeed, is given by Gadisden and Berenger and others, but to that of washing the penis with a mixture of water and vinegar by any one, "*qui timet de excoriatione et arsura virgae post coitum*, and *si quis vult membrum ab omni corruptione servare cum a muliere recedit*." Of the quotations from Glanville we have already spoken. One of these mistakes is noticed by Dr Titley; and Mr Bacot will doubtless, on re-perusing the authorities, take care to rectify them.

Upon the whole, Mr Bacot infers, that, if any historical fact is well established, it is, that the first appearance of syphilis is referable to the concluding years of the fifteenth century. But he regards it as by no means established that the disease was derived by importation from the West Indies or the American continent. The train of reasoning by which he arrives at this result is so similar to that employed by Thiene and Huber, that it is unnecessary to repeat it.

Dr Titley pursues a course somewhat different. From the passages which have at various periods been brought forward from various writers of antiquity, and also from others from Hippocrates himself, he contends that syphilis was not unknown to the ancients; and he regards the passages from Lanfranc, Saliceto, Bernard Gordon, Guido de Cauliaco, Arden, and Gascoigne, as sufficient evidence to demonstrate the existence of venereal diseases,—that is, we presume, foul complaints resulting from sexual intercourse, during the middle ages and previous to the date assigned for the appearance of syphilis in Europe. Upon this point it is sufficient to remark, that during the middle ages not only was sexual intercourse so indiscriminate, attended by circumstances so licentious, and accompanied with so little attention to personal cleanliness, that, if the disease really existed, this cause was in full operation; but so many foul diseases prevailed, that at this distance of time it is impossible, perhaps, to obtain direct or satisfactory evidence of the exact nature of any one of the disorders which are said to prevail, and much less on one which all must have been more or less anxious to conceal, and which they had good reason for regarding as a sort of punishment of their vicious habits. The manner in which he explains the origin of syphilis shall presently be noticed.

If, therefore, the American origin of the disease is either to be doubted, or to be positively denied, it comes next to be asked, whence did the disease originate? Was it in existence before, or even, as some have contended, was it known to the nations of antiquity? and if so, what are the circumstances *which kept* it in a latent or quiescent condition so long, and what are the circumstances which favoured its eruption, invested it with characters so formidable, and made it spread with rapidity so unrivalled? Was it the consequence of a new morbid poison? and if so, how did this happen to be developed at the period which the disease appeared?

We believe it must be admitted, that, previous to the appearance of the new disease at the close of the 15th century, there existed various morbid states of the genital organs, and more especially a discharge from the mucous surfaces, attended with scalding. To this, in all probability, are to be ascribed the complaints mentioned during the middle ages under the name of *arsura*, *scolagione*, or *brenning*, and which Beckett showed existed previous to the appearance of the peculiar new disease. This is admitted both by Thiene and Huber; and the former goes so far as to admit a *gonorrhœa* of antiquity, and a *gonorrhœa* of the middle ages. Into the proofs regarding the existence of the former, which are given in his second letter, we have no leisure to enter; but we may remark, that it makes little for the question at issue; and it is sufficient that the existence of a puriform discharge from the mucous surfaces of the generative organs in both sexes, previous to the appearance of syphilis, is satisfactorily established. The authorities which he adduces in his third letter, and which are either the same or of the same tendency with those quoted by Beckett and Friend, are competent to demonstrate the fact. It is not difficult, however, to see that since gonorrhœa has for a long period been admitted upon good grounds to be a disease quite distinct from syphilis and its effects, this tends in no degree either to explain the origin of syphilis, or to show that it was in existence previous to the 15th century.

All our authors agree in the general fact, that a disease, peculiarly contagious, and resulting from sexual intercouse chiefly, though also propagated by other modes of contact, appeared at the close of the 15th century. Waiving at present any disquisition as to the characters of this disorder, which, however, is closely connected with the history of its propagation, Thiene maintains, that, if not pestilential, it was epidemic, and, if not positively generated by the atmospheric constitution, that by this its characters and propagation were greatly influenced. To the operation of this cause he ascribes the general prevalence, and the great severity of the eruptive or cutaneous part of the disease; and since this, according to the testimony of all authors, underwent a remarkable mitigation towards the year 1520, and subsequently, both in frequency and intensity, he infers, that by this time the epidemic constitution had ceased to operate. The virulent principle or poisonous agent was still in existence, he argues; and, if its effects were less generally observed and milder, this was chiefly to be attributed to the smaller susceptibility resulting from the influence of the air on the human constitution. He further, however, maintains, that the disease has a particular tendency to affect the organs of generation and of voice, in consequence of the sympathy which subsists between them.

Huber give views somewhat different. From the facts adduced he infers, that the proper syphilis was in its origin a new disease; that it appeared in different countries of Europe, but by no means so extensively as has been asserted; and, lastly, that the majority of the morbid states whioh have been comprehended under the name of syphilis from the beginning of the 16th century, and much later to our own times, were evidently known and described by physicians before the appearancc of syphilis. When these circumstances are established, it should by no means result that syphilis might not exist in many other countries and be propagated in many places, of which positive accounts are wanting. He thinks, therefore, it may justly be concluded, that it existed in a sporadic form, known only to physicians, and that it was only when it became epidemic and was confounded with other foul cutaneous disorders, that it attracted general attention from historians and annalists.

On this point Mr Bacot, after proving that the disease did not originate in America, satisfies himself by acknowledging his inability to determine the real source of the disorder. He thinks that it is highly probable that it was first noticed among the Spaniards, and that by them it was conveyed to the southern part of Europe.

Dr Titley, on the other hand, regards it as established, that the new and contagious disease of the 15th century was the same as the Epian or the yaws of America. Whether this prevailed much or extensively, he thinks is still open to inquiry. On the other hand, he thinks that *lues venerea* was not in any of its forms new to Europe at or near the epoch mentioned, and that it has been known in the world from very high antiquity. He does not agree with Mr Beckett in regarding it as the same as leprosy; but he thinks it was known by the authorities which that author quotes, especially by John of Gadisden. He then proceds to inquire whether *lues venerea* is a disease to which it is possible to assign a distinct nosological existence; but on this point we shall not at present enter, since it involves a number of other considerations.

Here, indeed, for the present, our limits compel us to pause, and defer the further consideration of the subjects of these works till a future opportunity. It is almost superfluous to remark, that, notwithstanding all the research with which we have shown the subject has been investigated, the fact as to the precise origin of the venereal disease is not determined. It is only shown that it did *not* originate—and was not exported from America,—but it remains yet to be shown whence it did originate. The Maranic origin, or that from the Portuguese Jews, so strenuously maintained by Gruner, is as little tenable as any other. That hypothesis has been already refuted by Sprengel, and we feel it unnecessary here to resume the question.

ART. VII.—*Clinique Chirurgicale de l'Hotel-Dieu. Jusqu' à quelle époque est-il possible d'opérer la réduction des Luxations?* Par M. MARX. *An Account of Cases in Clinical Surgery collected at the Hotel-Dieu of Paris, and chiefly in the Wards of M. Dupuytren, illustrative of the question as to the interval after which Dislocations may be reduced.* By M. MARX. Memoir of pp. 60, 4to. Published in the Répertoire Général d' Anatomie et de Physiologie Pathologiques, &c. First Trimestre, 1829.

AMIDST the numerous investigations which have lately been made in the various departments of surgery, the subject of the reduction of old dislocations appears to have been unduly neglected. This may have arisen in part from cases of old unreduced dislocation being much more rare now than formerly in consequence of the improved state of surgical knowledge in all grades of the profession. But an equally efficient reason perhaps is, that most modern authorities in surgery have declared themselves averse to interfering with cases of the kind. *Mr Samuel Cooper*, who, in consequence of his extensive literary researches, and the impartiality of his narrative, may be said to express the opinion of at least all British surgeons on the question of reduction in old cases, after stating the results of the experience of Dessault in the latter years of his practice, observes that " when the bone has been out of its place more than a month, success is rarely obtained," and refers to some instances, where the immoderate and protracted force required to effect the reduction occasioned great mischief and even death.* *Sir Astley Cooper* seems also opposed to attempts at reduction after the bone has been long displaced. He states that at three months reduction of the humerus is hazardous, and that of the femur at eight weeks, and altogether his general expressions would discountenance attempts even at earlier periods.* The experience of *Dessault*, however, who several times succeeded in reducing dislocations of thirty or thirty-five days' standing, and even occasionally after an interval of two months and a half, or three months,† has encouraged several of his countrymen to follow his example; and at the Hotel-Dieu of Paris in particular most of the surgeons of the hospital, and especially M. Dupuytren, have repeatedly performed the operation of reduction with success at equally remote periods.

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* Surgical Dictionary, 1825, p. 388.

In the third volume of the Répertoire Général, *M. Flaubert*, chief surgeon of the Hotel-Dieu of Rouen, published several cases of an opposite tendency. These cases, which are five in number, show that in the course of his experience reduction of the shoulder-joint after eleven and fifteen days, five weeks and seven weeks, and reduction of the elbow-joint of twenty-seven days' standing have been attempted with invariable bad success. The first patient died of rupture of the axillary artery. The second and third were seized immediately after the operation with palsy of the arm, from which the former scarcely recovered at all, and the latter but very imperfectly; and hence the brachial nerves must have been injured. The fourth was attacked with palsy of the entire side, and died of inflammation and softening of the spinal chord in the neck, produced by the four lowest branches of nerves which form the brachial plexus having been torn out of the spinal chord by the roots. And the fifth suffered laceration of all the soft parts around the elbow-joint and probably of the arteries also, and lost entirely the use of the fore-arm.

These examples are certainly by no means encouraging. But M. Marx, the compiler of the present report, insists that the unfortunate issue of M. Flaubert's cases cannot be reasonably ascribed to circumstances inherent in the operation itself, or indeed to any thing else than the accidental occurrence of misfortune, such as the best surgeons must often have remarked in the course of their practice in even less formidable operations. Such he considers a legitimate inference from the cases he has collected in the clinical wards of M. Dupuytren and his colleagues at the Hotel-Dieu of Paris. These cases, of some of which we shall now present an abstract, show that old dislocations may be frequently reduced at so late a period as eighty-two days and even more; that this may generally be accomplished without much torture to the patient; and that, with the aid of proper precautionary measures, none of the serious accidents ever occur which

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* On Dislocations, p. 36.

† Œuvres Chirurgicales, par Bichat, i.

M. Flaubert experienced in the course of his practice. The precautionary measures here alluded to are the following: In every instance the warm-bath is used immediately before the attempt at reduction is made. The joint is also covered for some time previously with poultices rendered narcotic by laudanum, or extract of monkshood, henbane, or nightshade. If the patient is young and vigorous, blood-letting is practised once or oftener, according to circumstances. During the operation, the surgeon takes care to distract the patient's attention by some powerful means from the suffering which he endures, or rather dreads. "The mere idea of pain or fear," says the author with great force and justice, "causes the muscles to contract, so as to afford a material impediment to the success of the attempts at reduction. But if care be taken, during the process of extension and the concomitant manipulation of the limb to distract his attention, to occupy his thoughts with personal matters, to converse with him on the subject of his trade, to pique his vanity, or even to appear irritated and angry with him, the muscles become suddenly relaxed, and the bone is allowed to resume its place." A material point gained by making the patient talk, is, that, independently of his volition being distracted, the chest cannot be preserved in that fixed state which is necessary for every violent exertion. This circumstance is overlooked by M. Marx.

The cases which are recorded to illustrate the practice of M. Dupuytren are thirty-one in number. In seven of these the joint had been dislocated for only seven days or less. In five the dislocation had existed between fifteen and eighteen days; in three between twenty-one and twenty-six days; in three between thirty-one and thirty-seven days; in five between forty-one and fifty-two days; in four between sixty and eighty-two days; in two for three months; and in two for two years. In one of the last cases reduction was not attempted; in another of seventy-six days' standing three attempts were unsuccessfully made to reduce a dislocation of the elbow; but in all the rest the operation succeeded completely. Of the twenty-nine successful cases, six are extracted from the experience of other surgeons, namely, one by *M. St André* of reduction on the sixteenth, two by *Dessault* on the forty-fifth and ninetieth, one by *Delamotte* on the sixtieth, one by *M. Sanson* on the ninety-eighth, and one from the Memoirs of the Academy of Surgery, where the reduction was accomplished at the end of the second year. The twenty-three successful cases which remain were treated by M. Dupuytren, and in these the duration of the dislocation varied from fifteen to eighty-two days. The following examples will illustrate his mode of procedure:

Case IX.—*Luxation of the fore-arm backwards.*—A cabriolet-driver, 32 years of age, fell violently with the palm of his left hand on the ground. Six hours afterwards the swelling around the elbow-joint was so great, that a surgeon who examined it did not discover that the joint was dislocated, and satisfied himself with applying moist compresses, and subsequently leeches. On the ninth day the swelling began to abate; and soon afterwards the true nature of the case was discovered. *On the sixteenth day*, when he entered the Hotel-Dieu, the fore-arm was in a state of almost complete extension; according to the patient's account, it had been so from the moment of the accident; and the slightest attempt to bend the joint caused great pain. Notwithstanding the unusual symptom of an extended state of the arm, it was impossible to mistake the nature of the injury. The olecranon was prominent, and bounded above by a depression: The head of the radius could also be felt projecting on the outside of the olecranon, and a little lower. At the inside of the joint the two condyles of the humerus could be felt lower down than the prominences of the radius and olecranum; and when the two condyles were seized with one hand, while the projecting mass formed by the olecranon and the head of the radius were grasped with the other, it was easy to make the arm and fore-arm move transversely on one another.—M. Dupuytren directed a large poultice to be applied over the joint till the moment of attempting the reduction, which he proceeded to do two days after the patient's admission. A pad being placed in the axilla, a cloth folded like a neckcloth was placed over it, and its two ends brought the one before, the other behind the chest, and fastened to an immoveable bar. Another cloth was fixed on the lower part of the fore-arm, which was previously protected from friction by linen spread with cerate. The ends of this cloth being held by several assistants, extension was performed in the direction of the axis of the arm. After a few efforts, M. Dupuytren meanwhile constantly engaging the patient in conversation, an articular crepitus was heard, which announced the accomplishment of the reduction. M. Dupuytren immediately bent the fore-arm, while the assistants continued the extension; and now it was easy to ascertain that all the parts were in their proper places. The joint was covered with moist compresses. A little swelling appeared, but without pain, and only for a few days. In eight days he was allowed to get out of bed with the arm in a sling; after this he was made to practise slight movements of flexion and extension, pronation and supination; and in thirteen days after the operation he left the hospital, with the swelling entirely subsided, and the power of motion returning rapidly.

Case XII.—*Luxation of the humerus inwards and downwards.*—A cook, 55 years of age, fell into a vault, the entrance of which was accidentally open before him; and, according to his own account, bruised his shoulder. Although he insisted that his arm was not raised from the side at the time of the fall, and that he had not tumbled on his elbow or palm, it was obvious that the humerus had been dislocated. The acromion was prominent; there was a depression under it; the deltoid was flattened; and a round hard body could be felt at the fore-part of the shoulder, immediately under the caracoid process. The motions of the arm were much restricted; and if an attempt was made to rotate it, the whole body was moved. An excoriation on the elbow proved that the patient's account of his accident was incorrect; indeed, it was impossible that such a dislocation could have been produced by the fall he described.—Immediately on his admission, *eighteen days after his accident*, M. Dupuytren proceeded to attempt reduction. The patient was fastened as in the last case, and extension was performed by six assistants, while M. Dupuytren, holding the humerus, tried to restore it to its proper place. The first trial failed entirely. A second also failed, the head of the bone being brought on the edge of the glenoid cavity, but no farther, as it could not enter the aperture in the capsular ligament. On discontinuing the extension, the head of the bone returned to its former place. On a third attempt being made, the head of the bone was again brought on the edge of the glenoid cavity; and then, the extension being continued, the elbow was suddenly brought close to the side, upon which the reduction took place without difficulty, and with the usual sound. On undoing the bandages, the parts constituting the joint were found to be in their proper places, and the patient had not even suffered any excoriation, or contusion from the cloths, notwithstanding the force employed. A little swelling which appeared on the hand was soon discussed; and in six days the patient left the hospital, with the power of moving his arm in every direction with facility and without pain.

Case XVI.—*Luxation of the femur upwards and backwards.*—A young man, a labourer, of slender form, was thrown while wrestling, and fell on his left side with the thigh bent, and crossing the opposite leg. He was unable to rise or move his leg, which he observed to be shorter than the other. A physician who saw him soon after did not discover the dislocation. In thirty days he applied at the Hotel-Dieu, at which time the left leg was three inches shorter than the other, was slightly bent, adducted, and turned very much inwards. The great trochanter was much higher and more prominent than that of the other leg, and along with the head of the femur formed two distinct projections on the outer part of the hip. The movements of extension, abduction, and rotation outwards, could not be executed. The patient could walk by means of crutches, and leaned slightly on the point of the foot of the affected side. It was obvious that he had a dislocation of the thigh-bone upwards and backwards. After examining him, M. Dupuytren assured him he should be cured, provided he had not given a false account of the date of the dislocation, like a patient not long before, who, being admitted into the Hotel-Dieu for another complaint, took advantage of the opportunity of requesting that an old dislocation might be reduced, and allowed himself to remain three quarters of an hour in the operator's hands, before confessing that the dislocation was of nine years' standing. The young labourer received on the evening of his admission an ounce of the syrup of poppies, and a poultice was placed over the haunch. Next day, the *thirty-first after the accident*, the reduction was accomplished. The first attempt failed; but at the second the head of the femur slipped into the cotyloid cavity with a shock and peculiar sound. The apparatus used in this instance consisted of a pad on the inside of the right groin,—a towel over the pad, with the ends passing, the one behind, the other before, and both secured to an immoveable ring,—a towel fixed above the ankle, which was protected from abrasion by linen spread with cerate,—and, lastly, another towel passed round the upper part of the affected haunch, and held by a single assistant to preserve the pelvis steady. After the head of the femur slipped into its proper receptacle, the leg was found to be several lines longer than the other, which was evidently owing to the swelling of the tissues covering the cotyloid cavity. The two legs were tied together. For twenty days he complained of occasional severe pain in the sole of the feet, which, however, had also annoyed him before the dislocation was reduced. On the twenty-fifth day he was allowed to leave his bed, and to walk about with the aid of crutches. In thirty-eight days he was discharged with as complete use of the left as of the right limb.

CASE XX.—*Luxation of the humerus forwards.*—A woman, 62 years of age, *six weeks before her admission* into the Hotel-Dieu, fell down stairs upon her right elbow. Although she felt much pain in the shoulder-joint for some days subsequently, no attempt was made to determine the nature of her accident. Gradually she regained the use of the arm to a limited extent. But the elbow always continued separated from the side; she could not move it forwards or to her mouth; and when the humerus was traced along its axis, it was obviously found to terminate at the fore and inner part of the axilla, immediately under the coracoid process of the scapula, where a hard round tumour could be felt, which was moved by attempts to rotate the arm. At the fore part of the shoulder, no depression could be felt under the acromion, but the deltoid could be pressed in with the finger, and at the back of the shoulder the depression was visible. This appeared to have been originally a case of luxation downwards, which in consequence of the patient's repeated and strong efforts to move the arm had become a dislocation forwards. On the forty-fifth day after the receipt of the accident, the usual precautionary measures having been taken, M. Sanson proceeded to attempt the reduction in the manner formerly specified. The assistants were directed to perform the extension in the acquired direction of the arm, that is downwards and outwards. The head of the bone being thus disengaged from its artificial situation, and pulled a little downwards, the direction of the extending power was changed to forwards and inwards, while M. Sanson endeavoured at the same time to move the head of the bone backwards and upwards. The first attempt failed; but the second, made with a little more force, succeeded. The arm was then kept for fifteen days bound to the trunk. Not long afterwards she was dismissed, and at this time she had the free use of her arm, and did not suffer any pain.

Case XXII.—*Luxation of the humerus upwards and forwards.*—An elderly decrepit woman fell with the whole weight of her body on the right elbow. Next day she was carried to the *Bureau Central des Hopitaux,* where a surgeon who examined her decided that she had merely sustained a severe contusion of the shoulder. Several weeks, however, having passed without any amendment in the movements of the joint, she repaired to the Hotel-Dieu, where M. Dupuytren at once recognized a dislocation upwards and forwards. The elbow was maintained at a distance from the side, the deltoid was flat, the acromion prominent, and the head of the humerus could be felt under the clavicle on the upper, anterior, and outer part of the chest.—The patient was directed to be bled, and to have the shoulder covered with emollient poultices. Next day, *the forty-ninth after the accident,* the reduction was attempted in the usual manner. The first attempt failed. The second was equally unsuccessful. At the third, M. Dupuytren, by pushing the head of the humerus downwards and outwards, succeeded in bringing it into the glenoid cavity. The right shoulder now appeared similar to the left; but as the head of the humerus projected slightly forwards, it was conceived that some portion of ligament or membrane was interposed between it and the glenoid surface. The elbow was therefore kept carefully fixed to the side and a little forwards, while the head of the bone was kept outwards and backwards by a proper pad in the axilla. In eight days the bone no longer projected forwards. In twenty-two days there was no longer any apparent trace of the dislocation, the shoulder was quite like that of the opposite side, and the patient could execute freely every movement of the joint. In four days more she was dismissed.

Case XXVI.—*Luxation of the femur upwards and outwards*—A quarryman, 23 years of age, was overwhelmed by the fall of a bank of earth, in consequence of which he received several wounds of the head, and lost one of his eyes. While under treatment for these injuries, it was discovered that he had also dislocated the left hip-joint. Five weeks after the accident, an attempt was made to reduce the dislocation, but without success. On afterwards repairing to the Hotel-Dieu, it was found that the knee and foot were turned much inwards,—that the thigh could not be abducted,—that the trochanter was more prominent and nearer the crest of the os ilium than that of the right side,—that the head of the bone could be felt farther behind in the hip,—and that the fold of the groin was deeper than natural, There was not much apparent shortening, because at the age of fourteen he had sustained a fracture of the neck of the right thigh-bone, which was followed by shortening of that limb to the amount of two inches. A bath was ordered, and on the day after his admission, *seventy-eight days after the receipt of the injury*, M. Dupuytren proceeded to reduce the dislocation. The apparatus for extension and counter-extension being arranged as formerly described, (case xvi.) the assistants began to pull at first in the direction which the limb had acquired, and afterwards more outwardly. At the same time M. Dupuytren pushed the trochanter downwards, and brought the knee outwards. By the first pulls the head of the bone was brought a little lower. The assistants then ceased to increase the force for a little, the extension, however, being steadily maintained; and then the pulling was renewed with increased power till the limb attained its proper length, and the joint its natural form. At this moment a slight crackle was heard. The bandages being then removed; the left was found to be two inches longer than the right leg. Notwithstanding the great force required to reduce so old a dislocation, there was only a slight excoriation of the skin caused in the right groin. The patient was kept completely at rest for some days, after which he began to walk with crutches; and in twenty-seven days he left the hospital perfectly well.

Case XXVII.—*Luxation of the humerus downwards, reduced on the eighty-second day.*—A washerwoman, 48 years of age, while carrying a basket of clothes on her shoulders, fell on her right side, the arm being at the moment separated from the side. She immediately felt great pain in the joint. Nevertheless, she resumed her employment in a few days. But as she found the motions of the arm very much impeded, she applied for relief at the Hotel-Dieu, *eighty-two days after the accident.* The right shoulder was flat, and the acromion prominent; through the substance of the pectoralis major muscle a hard round tumour could be felt; behind the shoulder there was a depression; the right arm was longer than the left, the elbow was directed backwards, and could not be brought close to the trunk, neither was she able to raise her hand to her head.—M. Dupuytren proceeded to the reduction. The first efforts were fruitless. But when another attempt was made with greater force, a distinct sound and sudden movement announced the return of the bone into its socket. All the symptoms just noticed at once disappeared. The shoulder became round, the motions of the joint free, and it became necessary to forbid the patient to practice them, so delighted was she at finding she had recovered them. The arm was bound to the side with the proper bandage. No unpleasant symptom whatever followed the operation, although the efforts required to accomplish the reduction were great, and three trials were made before it was accomplished. In fifteen days the bandage was removed. In forty-five days the patient left the hospital, with the arm still somewhat feeble, but with the motions of the joint all perfect.

To these notices we shall merely add that M. Sanson, in 1827, reduced a dislocation of the humerus *of ninety-eight days' standing*, or sixteen days older than the instance last described; and that the reduction was effected at the second trial without injury to the patient. This case is recorded in the *Elémens de Pathologie Médico-Chirurgicale* of MM. Roche and Sanson, and is briefly noticed in the report of M. Marx. An instance of alleged reduction of the hip-joint at the distance of two years, related by M. Guyenot, in the fifth volume of the Memoirs of the Royal Academy of Surgery, attested by Louis, Tissot, and Cabanis, and copied in M. Marx's report, is justly held by him to be equivocal. M. Sanson's case is therefore the oldest successful instance authentically recorded.

The whole paper now analyzed shows that the reduction of old dislocations is in skilful hands much less formidable and much more frequently successful than the surgical profession have usually believed; and therefore deserves their serious attention.

ART. VIII.—*Remarks on the Tendency to Calculous Diseases; with Observations on the Nature of Urinary Concretions, and an Analysis of a large part of the Collection belonging to the Norfolk and Norwich Hospital.* By JOHN YELLOLY, M. D. F. R. S. &c. (*From the Philosophical Transactions.* London, 1829.)

THIS paper is, to the medical reader, the most valuable that has for a long time appeared in the Transactions. Though the paucity of materials restricts Dr Yelloly to little more than the commencement of the subject of which he treats, he unfolds views of the etiology of urinary disorders which merit particular attention.

The first attempt to ascertain the comparative frequency of calculous diseases in different districts of England was made by Dr Dobson in 1779; and though one of the results of his inquiries was, that the proportion of calculous cases in the Norwich Infirmary was about thirty times as great as in the Cambridge Hospital, it appeared that in other parts of England the prevalence of these diseases was, on the whole, very uniform. Thus, in the Gloucester, Worcester, Hereford, and Exeter Hospitals, the proportion of stone cases was 1 in 394 patients. In the north-east districts, including the hospitals of York, Newcastle, Leeds, and Manchester, it was 1 in 420. In the north-west districts again, consisting of the hospitals of Liverpool, Chester, and Shrewsbury, and the whole of North Wales, the proportion was only 1 in 3223. Some of this disparity Dr Dobson imputed to the influence of cyder in the production of the disease; but perhaps not on very good grounds.

The next attempt of this description was made by Dr Marcet, who found, among other results, that the Norwich Hospital retained in this respect the superiority which it had acquired in the time of Dr Dobson. In an average of annual admissions, amounting to 428, it was found that the operation of lithotomy was performed on 1 in every 38, or rather oftener than eleven times annually,—a degree of comparative frequency, much greater than in any of the hospitals of the metropolis. Dr Marcet found, for instance, that in St Thomas's Hospital, for the ten years preceding 1819, the annual average of operations was 5½, or 11 in two years, or not half the annual number of the Norwich Hospital; and if the admissions of the two hospitals are contrasted, not one-seventh of the proportion of operations. In St Bartholomew's, with annual admissions to the amount of 3760, the annual average amounted to 11 cases, or 1 in each 340 patients; or about $\frac{1}{9}$ of the proportion of operations. In Guy's, with annual admissions to the amount of 2637, the annual average of operations varied for twenty or thirty years previous to 1819, from 9 to 10, or in the ratio of 1 in about 300 patients.

The defect of evidence which the researches of Dr Marcet showed to exist upon this subject induced Mr Smith of Bristol to institute an extensive inquiry into the frequency of calculus in the different counties of Great Britain and Ireland, the details of which are found in the 11th volume of the Medico-Chirurgical Transactions. The general result is, that, of the cases positively reported, 60 are annually operated on in the counties of England and Wales; and allowing one-half for those not recorded, which is perhaps above the truth, there are 90 for county operations. In the metropolis the number of cases operated on in hospitals amounts on an average to 47 annually; and allowing 30 for private cases, the total amount of operations performed in London is rated at about 77. In Scotland, Mr Smith makes the annual average to be no more than 12 for the whole country, with a population of 1,600,000; and the same number for Ireland, with a population of 4,250,000, neither being more than the annual average of the Norwich Hospital. Norfolk is the county in which the greatest annual number are cut, being 11½ annually; Yorkshire is next, being 8 or 9 annually,—one-third of these being cut in Halifax, and about one-half in Leeds. Lancashire is about 4; and the rest vary from 2 and a fraction to 1 or less. The total amount of stone cases in the whole population of 21,000,000 is about 200, or 1 in 105,000. Norfolk and Suffolk are the most lithophorous districts, being 1 in every 4577.

Upon this statement Dr Yelloly makes some modification and addition. From the registers of the London hospital, to which he was many years physician, he finds, that of the calculous cases admitted, two-thirds are furnished by the metropolis, and one-third by the country; and, allowing Mr Smith's estimate of 47 cases to be reasonably near the truth, he refers 31 of these cases to the city population, amounting to rather more than 1,200,000 inhabitants, and the remaining 16 to the population of the surrounding counties, in which there are either no hospital, or hospitals too recently established to affect the calculation. The number of public operations, according to Mr Smith's estimate for England and Wales, amounts to $60 + 47 = 107$, to which Dr Yelloly adds 4 for Suffolk, which has, since the date of Mr Smith's inquiry, been provided with an hospital, making 111 operations annually in a population of nearly 12,000,000, or 1 case for every 108,000 inhabitants. If, however, from this estimate a deduction be made for the Norfolk and Suffolk cases, that is $111 - (11\frac{1}{2} + 4 = 15\frac{1}{2}) = 95\frac{1}{2}$, there is only 1 calculous case for every 118,000 inhabitants; and, making the still further deduction of the cases furnished by London and the adjoining districts, with the respective population, or $95\frac{1}{2} - 47 = 48\frac{1}{2}$ upon a population of 9,015,000, it gives only one in 186,000 * for the whole remaining population of England and Wales, or about one-fifth of the proportion of London and of the Norfolk district, excluding Norwich, and about one-ninth of the proportion of Norwich itself. From these calculations the curious result is obtained, that, though the Norfolk and Suffolk district, and the counties contiguous to London are highly prolific of stone, the tendency to calculous complaints in the kingdom at large is small indeed.

By taking individuals between the ages of 14 and the most extended period of active exertion in adult age, the number of calculous cases may be still farther reduced. The returns show that nearly one-half of the whole population consists of persons between those ages; yet the calculous cases belonging to this period of life, as deduced from the Norwich register, are not a third of the total number. From these considerations Dr Yelloly experiences difficulty in assenting to the opinion defended by Mr Copland Hutchison, that among those following a maritime life, urinary calculus is not a frequent disease. He admits, however, that, throwing aside the London and Norfolk districts as unusually prolific of the complaint, and comprehending Ireland and Scotland, the general result will be nearly as stated by Mr Hutchison.

From the Norwich as well as the London registers, it results that the liability to calculus is nearly as great during the first ten years as at any future period of life; and taking the cases afforded by Norwich and London, excluding those of the country districts, as many cases are observed below as above 14,—showing that the proportion of children affected with the disease is greater in towns than in the country.

Regarding the mortality from the operation of lithotomy, the statements vary. That of Cheselden is generally supposed to be so low as 1 in $10\frac{1}{2}$; but if the whole of his early cases be included, Dr Yelloly infers that the average mortality of his hospital practice must amount to 1 in 8.6. Reckoning from the first operations in the Norfolk and Norwich hospitals, the mortality amounts to 1 in 7.29 cases; but it is a proof of the improvement of surgery that it has been reduced of late to 1 in 8.42, which is not very different from that of Cheselden. In this calculation it is interesting to remark, that in early life the mortality is much smaller than in adult age; up to the age of 14 the deaths are only 1 in $14\frac{1}{2}$; after that age they are 1 in

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* Dr Yelloly makes it 188,000. But after making the calculation, we find it to be what is above stated.

$5\frac{1}{4}$. Between 14 and 40 they are 1 in $10\frac{1}{2}$; and after the latter period they rise to 1 in $3\frac{3}{4}$. To exhibit the number of operations at the Norfolk and Norwich hospital with the relative mortality at different periods of life, Dr Yelloly gives the two following tables:—

Age or Sex.	Operations.	Cured.	Died.	Mortality.
Both sexes........	649	560	89	1 in 7.29
Males	618	531	87	1 — 7.1
Females	31	29	2	1 — 15.5
Both sexes.				
Under 14	292	272	20	1 — 14.6
14 and upwards	357	288	69	1 — 5.17
14 to 40	155	140	15	1 — 10.33
40 and upwards	202	148	54	1 — 3.74
14 to 50	196	171	25	1 — 7.84
50 and upwards	161	117	44	1 — 3.56
Under 16	317	294	23	1 — 13.78
16 and upwards	332	266	66	1 — 5.03

Age.	Operations.	Cured.	Died.	Mortality.
Inf. to 10	255	237	18	1 in 4.16
10 — 14	37	35	2	1 — 18.5
14 — 20	62	55	7	1 — 18.85
20 — 30	47	42	5	1 — 9.4
30 — 40	46	43	3	1 — 15.33
40 — 50	41	31	10	1 — 4.1
50 — 60	92	69	23	1 — 4
60 — 70	63	43	20	1 — 3.15
70 — 80	6	5	1	1 — 6

Something of the mortality of lithotomy has been generally understood to depend on the size of the concretion, which is always more likely to inflict injury in removal the larger it is. To demonstrate the exact influence of this cause, Dr Yelloly finds that of 52 cases of adult males cut at the Norwich hospital, in which concretions of 2 oz. or more were extracted, 31 died, or nearly 2 in 3; while in 282 cases, also of adult males, in which the concretions were under 2 oz., the mortality was only 37, or less than 1 in 7. From these facts two practical rules may be deduced; the first, that early operation as soon as a calculus is recognized, and before it has had time to attain a great bulk, is always most favourable to the successful issue; and the second, that, if the stone is too large for easy extraction by the usual incisions, the instrument of Mr Earle should be employed.

The most usual cause of fatal termination, excluding the circumstances now mentioned, Dr Yelloly is inclined to look for in the constitutional shock inflicted by the operation. It is a remarkable instance of the equalizing principle in the mortality of this operation, that, of the two most distinguished contemporary lithotomists of the Norfolk and Norwich hospital, the late Dr Rigby and Mr Martineau, the first lost only 3 of his first 50 cases, or 1 in $16\frac{2}{3}$, and 12 of the remaining 56, or 1 in $4\frac{3}{4}$, making the average mortality below 1 in 7; and conversely, the latter lost 11 of his first 50 cases, or 1 in $4\frac{1}{2}$, and only 6 of the remaining 97, or 1 in $16\frac{1}{6}$, making his average mortality less than 1 in 8. The latter gentleman, it is well known, ascribed his success in the recent period of his career to the substitution of the knife for the gorget in dividing the neck of the bladder. The blunt gorget he used as a guide to the forceps.

Recurrences of stone are rare. The records of the Norfolk and Norwich institution give 14 instances, 3 below and 9 above 14, in the whole number of cases, or 1 in 46, of the operation being twice performed on the same individual. Two of the cases above 14 died. In 4 cases the formation of the second calculus took place within one year; in 5 within two years; in 3 within three years; and in the remaining 2 cases the operation became necessary only after a lapse of seven and ten years respectively. In such instances the secondary calculus is not necessarily of the same character as the first. It may, if in a child, consist of cystic oxide at first, and another substance afterwards. In some instances even the characters of the deposition may be still more variable. Dr Yelloly mentions a case at the Cambridge hospital, in which a concretion of cystic oxide had a lithic nucleus, and, upon the extraction of which, another was speedily formed, consisting of a fusible exterior, and a lithic interior, and requiring the operation to be repeated in less than a year.

On the comparative frequency of the disease in the higher and lower ranks of society, little correct information can be obtained. From the experience of Mr Martineau and that of Mr Trye, as reported in Mr Smith's paper, Dr Yelloly is disposed to regard 1 of the former to 11 of the latter, as not far from the truth.

On the influence of the circumstances of food, drink, soil, or habits, Dr Yelloly feels it difficult to advance any thing very positive. The greater frequency of the disease in Norfolk might seem to be in some degree chargeable to the employment of badly fermented farinaceous food, which is largely used by the peasantry of the country. But this inference may be directly opposed by the fact, that in Scotland and the north of England, worse fermented articles of food in rye, barley, oats, and pease have been long in use without producing the same effects. Dr Yelloly justly remarks that the laxative tendency of some of these articles may obviate the disadvantages which their acescent influence might lead the physician to anticipate. On this point conjecture is doubtless misplaced. But when the observations made by Magendie on the share which intemperance and the use of indigestible substances have on the production of gravel, are combined with some of the ingenious views of Prout on the circumstances which favour the lithic deposition, we think we should pause before we ascribe to any kind of farinaceous food the power of favouring the formation of urinary concretions. Upon the whole, Dr Yelloly infers that the only positive conclusion which can be drawn is, that "the tendency to produce calculous complaints is greater in towns than in the country;" and if this be established as a general result, it seems to indicate the existence, in children particularly, of a connection between some diathesis which prevails in towns, probably the scrofulous, and the tendency to the deposition of lithic acid, on which the origin of urinary calculi greatly depends. The following table indicates the relation of calculous complaints to population in the instances examined by Dr Yelloly.

Place.	Population.	Number of Stone Cases.	Cases per Annum.	Comparative Frequency.
Norwich . . .	50,000	128 in 56 yrs.	2.28	1 in 21,000
Norfolk, including Norwich	351,000	575 ———	10.26	1 — 34,000
Norfolk, excluding Norwich	301,000	447 ———	7.98	1 — 38,000
Suffolk	234,000		5.26	1 — 44,000
Norfolk and Suffolk, including Norwich . .	585,000		15.5	1 — 37,000
Norfolk and Suffolk, excluding Norwich . .	535,000		13.33	1 — 41,000
London . . .	1,200,000		31.	1 — 38,000
Adjacent Counties . .	1,200,000		16.	1 — 76,000
England and Wales .	12,000,000		111.	1 — 108,000
England and Wales, excluding Norfolk and Suffolk	11,415,000		95.5	1 — 118,000
England and Wales, excluding Norfolk, Suffolk, and London, with its adjacent counties . . .	9,015,000		49.	1 — 188,000
Do. persons between 14 and 50	4,134,000		14.7	1 — 280,000
Bristol and its liberties .	87,000	173 — 82 yrs.	2.1	1 — 41,000
Bristol country district .	750,000	181 ———	2.2	1 — 340,000
Scotland . . .	2,000,000		8.	1 — 250,000
Dundee . . .	30,000	26 — 36 yrs.	.86	1 — 41,300

When Dr Marcet examined the Norwich collection, none of the calculi contained in it were divided; and his experiments were necessarily confined, unless in the instance of those which had been broken in extraction, to the outer surface,—a method, unfortunately, from which little information on the structure of the concretion can be obtained. The division of a certain proportion of these during the last five years, amounting with those broken to about 330, rather more than half the collection, which consists of 649, or at the end of 1828 of 650 specimens, has enabled Dr Yelloly in some measure to supply this deficiency. The results are exhibited in the following table:—

Calculi consisting principally of one deposit.

Lithic acid, . . .	81
Lithate of ammonia .	20
Oxalate of lime, . . .	20
Phosphate of lime, . .	4
Fusible calculus or mixed phosphates,	37

Calculi consisting of two deposits.

Lithic acid and lith. of am. .	37
——— oxal. of lime, .	11
——— mixed phosph.	10
——— phosph. of lime, .	2
Lithate of am. and lithic acid,	2
——— ox of lime, . .	25
——— mixed phosph. .	14
——— phosph. of lime, .	1
Oxalate of lime and lithic acid,	10
——— lith. of ammonia . .	1
——— mixed phosph. .	15
——— phosphate of lime, .	3
Mixed phosphate and phosph. of lime,	2

Calculi consisting of three deposits.

Lithic acid, phosphate of lime, and mixed phosphates . .	2
——— oxalate of lime and phosph. of lime, . . .	1
Lithic oxalate of lime and lithic of ammonia.	2
——— oxalate of lime and lithic acid,	4
——— lith. of ammonia and ox. of lime, . . .	2
——— oxal. of lime and mixed phosph.	1
Lithate of ammon., oxalate of lime, and mixed phosph. . .	3
——— oxalate of lime and lithic acid,	8
——— phosphate of lime and lithate of ammon., . .	1
——— lithic acid, and mixed phosph.	2
Oxalate of lime, lithic acid, and lith. of ammonia, . .	1
——— lithic acid, and oxalate of lime,	1
——— lithic acid and mixed phosph.	2

Calculi consisting principally of four or more deposits.

Lithate of ammonia, oxalate of lime, lithic acid, and mixed phosphates,	1
Oxalate of lime, lithic acid, oxalate of lime, and mixed phosphates, .	1
Lithate of ammonia, oxalate of lime, phosphate of lime, oxalate of lime, and lithate of ammonia. .	1
Total,	328

In speaking of the lithate of ammonia calculus, Dr Yelloly has occasion to differ from Mr Brande, who ascribed the evolution of ammonia in all instances to the decomposition of the ammoniacal salts, more especially of the ammoniaco-magnesian phosphate. From the effects of re-agents, on the contrary, he infers that the ammonia thus evolved is actually in combination with the lithic acid itself. He confirms, however, the statement of Mr Brande, that muriate of ammonia is constantly combined with the lithate of ammonia calculus; and he further finds this salt with the lithic acid, the mixed phosphates, and calculi of oxalate and phosphate of lime, and, though in very minute proportions, in the cystic oxide concretion.

The lithic calculi form one-third of the number analyzed of the Norfolk collection; and when this number is augmented by those which have lithic acid, or lithate of ammonia as a nucleus, it results that 238 of 328, or nearly three-fourths of the whole, are of this constitution. This corresponds with the results hitherto given of other collections. Thus in the analysis of 187 calculi by Dr Henry, 158 or five-sixths were either lithic, or deposited on lithic nuclei. In the Cambridge Hospital the number is 27 in 41; and in the Leyden collection it consists of 38 in 49, or three-fourths of the whole. These facts demonstrate, that whatever peculiarities of soil, climate, or mode of life distinguish each country, the general result as to the predominance of the lithic diathesis is much the same.

Though the usual appearance which the oxalate of lime gives a calculus is the tubercular or nodular, like a mulberry, it occasionally assumes the appearance of bright amber-coloured or transparent white crystals of a flat octahedral shape. The Norwich collection contains not fewer than twenty examples of this kind of crystallization.

In the bone-earth calculi, Dr Yelloly was led to suspect the existence of carbonate of lime; and this suspicion, which he has experimentally verified, is also confirmed by the researches of Dr Prout and M. Proust. This applies not only to vesical but to prostatic calculi. The same substance has been also found by Dr Prout and the author in two calculi of the mixed phosphates in the possession of the former, and in several of those in the Hunterian collection. Carbonate of lime, we may add, was found in human calculi by Dr Gilby of Clifton, and Mr Smith of Bristol.

These statements furnish some curious and interesting views relative to the original formation of urinary concretions, and also to the changes which, from time to time, take place in the calculous diathesis. Though the author, with great modesty, admits that the calculations can only be regarded as approximating to the truth, the facts which he adduces, and the general results which he elicits from them, form the basis of an inquiry not only of the greatest interest, but, we trust, susceptible of very useful practical applications. The etiology of calculous diseases is still involved in the greatest obscurity; and, if we ever hope to dispel this, it must be by such inquiries as that on which Dr Yelloly has now entered. In order to supply proper materials for such an inquiry, accurate registers ought to be kept in all country and city hospitals; and we cannot allow the present opportunity to pass without recommending for general imitation the example of the Norfolk and Norwich Institution. It is easy to see how much valuable information might have been preserved, if this system of registry had been adopted in the hospitals of the metropolis.

We could have wished, that, by combining with his own the views of Magendie, and the interesting researches of Dr Prout, Dr Yelloly had attempted to advance a step farther in attempting to ascertain the connection between the remote and the proximate causes of the disease. It is one of the curious results, for instance, of the analytical labours of Dr Prout, that the elementary atoms of urea and lithic acid are in the relation of multiples or submultiples of each other; and since this shows the connection in composition, and what may be termed the interchange of elements, the chief object of the inquiry should be to determine the circumstances of the human frame on which the occurrence of this interchange depends. Thus, though both have the same quantity of oxygen, lithic acid contains one-third of the hydrogen which urea does, double the quantity of carbon, and about one-third less of azote. The change then consists in the carbon being doubled, the hydrogen reduced to one-third, and the azote being reduced about one-third. The mode in which this is effected is the object to be determined. Dr Prout has himself entered on this inquiry; but he has merely entered on it; and, until some advance be made, we must remain as much in the dark as ever. It must be chiefly to him, and to the ingenious author of the present paper, that the profession can look for the elucidation of this difficult and important question; and we cannot conclude these remarks, without expressing the hope that either or both will resume the subject, and continue an investigation which is so important.

Art. IX.—*A Treatise on Poisons, in relation to Medical Jurisprudence, Physiology, and the Practice of Physic.* By Robert Christison, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c. &c.

In giving an account of the volume before us, we have a delicate task to perform. Our readers probably already know, and if not, they will soon know, the relation in which the author and critic stand to each other. Whether we shall bestow praise or censure, we can scarcely hope to get credit for impartiality. We shall, however, conscientiously endeavour to discharge ourselves of all feelings of private regard, and, what is perhaps more difficult, of all professional jealousy, which the perusal of this work might have excited in a person placed in our situation. Since the days of Homer it has been a constant complaint, that the physical powers of each successive generation were inferior to those of that which preceded it. But in regard to intellectual power we, now in the thirty-seventh year of our critical function, must admit the "march of intellect" to be no joke, but a most serious truth. Considering Dr Christison as in some measure our professional son, we feel proud to acknowledge that he has effected what we at no time were able to perform, and, reversing the original meaning, to say of him, "Sequiturque patrem non passibus æquis."

Having thus thrown aside the mask, as the best warrandice against undue partiality, we shall now proceed to business.

Legal medicine has long been with us a favourite subject of study. We rejoice that its importance is now beginning to be more duly appreciated, to which our pages have contributed not a little. It is, indeed, on many accounts, an important branch of our professional study; for, besides the exercise of our art in the prevention and cure of diseases, and in performing surgical and obstetrical operations, legal medicine is the only practical application of our professional knowledge. Anatomy, chemistry, medical institutes, and pharmacology, are but preliminary studies to fit us for the practice of physic, surgery, and legal medicine. Our profession has been habitually, though erroneously, reproached with its being uncertain, and engaged in vague and frivolous investigations,—but to legal medicine at least these objections cannot apply. Its facts must be precise and indisputable. It admits of nothing vague or conjectural, and its reasonings must be clear, and conducted according to the severest laws of induction. It brings forward in a prominent point of view whatever is certain in physiology, pathology, and therapeutics.

Of the variety of subjects which legal medicine embraces, the science of Toxicology is one of the most important, whether we regard the interest of the individual cases to which it is applicable, or the frequency of their occurrence. Indeed it is surprising, that till the appearance of Dr Christison's volume, no original work on toxicology has been published in this country. For although the facts upon which it must be founded, and the inductions derived from them are of all countries, yet the technical legal process is different in each, and, although the physician has not to interfere with the duties of the lawyer, the kind of evidence admissible in British courts, which the medical jurist has to cultivate, differs in many respects from that desired in foreign tribunals.

But, if our professional literature has long been destitute of a classical work on toxicology, that defect no longer exists, and we scarcely fear to be accused of national prejudice or partiality, when we declare our decided preference of the volume before us, to the most valuable productions of Germany or France. In giving this opinion, we have not the most remote intention of undervaluing the labours of Dr Christison's predecessors, for without their assistance he could not have produced a work like the present, and he would have merited no praise, if he had added nothing to the information already communicated by them, or in no way improved the scientific exposition of the subject.

In a preface somewhat more substantial than usual, Dr Christison expounds at some length the objects of the science of toxicology, which is fourfold. It supplies antidotes for the various poisons—it furnishes the physiologist with valuable instruments of research in his investigations into the laws of the animal economy—it aids the physician in his inquiries into the action of many energetic drugs,—and it collects from the numerous branches of medical knowledge, as well as from collateral sciences, the materials of the most important department of medical jurisprudence.

The idea of this work was first happily suggested to Dr Christison, in consequence of his becoming sensible, in the course of a very extensive practice in toxicology, of the deficiencies in the works upon the subject enjoying the greatest reputation. When poisoning is suspected, the aid of the physician may be required on two occasions—in the investigations carried on by the Sheriff in Scotland, or Coroner in England—and on trials before the high criminal courts. As it is of importance to every member of the profession, whether he may have studied medical jurisprudence or not, to be aware of the duties he may be suddenly called upon to perform, and of what is expected of him, we shall quote some passages from our author on the subject.

" In the former, (the preliminary investigation,) the purpose of his opinion is commonly to determine the propriety of farther proceedings. Here he has very often to inquire into cases which are really instances of natural death, but which in peculiar circumstances have given rise to a suspicion of poisoning. Hence, while he must on the one hand be prepared to prove in some cases that death has arisen from poison, he must on the other hand be also sometimes ready, not only to give the mere negative opinion, that there is no satisfactory proof of poisoning, but likewise that death has positively arisen from natural disease. In cases of this kind, it often happens that no particular poison is indicated. There is merely a general suspicion or charge of poisoning of some kind or another; and even although no particular poison may be found, the physician is required to say, whether there is a certainty, probability, or possibility of poisoning *in a general sense.*

" For the elucidation of these questions, which have received but little attention from authors, I have dwelt at considerable length in the present treatise on the Evidence of General Poisoning."—P. viii.

" On trials before the high criminal courts the medical witness is somewhat differently placed. While he ought to be always prepared to give full effect to every medical circumstance favourable to the prisoner, he should at the same time recollect that very few trials indeed take place in Britain, where it is not in the highest degree probable that poison was given; and consequently, that the main purpose of his inquiries must be to bring together the whole medical evidence to this effect, and to secure it against the doubts which the ingenuity of counsel is sure to throw over his conclusions, if the premises are any where false or defective.

" On such trials the prisoner is usually charged with administering a particular poison, and also some poison unknown to the prosecutor. In some instances the evidence of the particular poison is merely presumptive; and the presumption may not be strong, so that the charge is substantially one of poisoning in a general sense.—P. ix.

"Much more frequently, however, on trials before the high criminal courts the evidence is pointedly directed towards proving the administration of a particular poison."—P. x.

As we do not intend in the present article to enter upon an examination of the more voluminous portion of this valuable work, treating of particular poisons, we shall merely allude to the sources of information which Dr Christison has brought to bear upon the subject.

Besides making very great use of his personal experience, which alone is sufficient to stamp a character of originality on the work, Dr Christison is no less original as a compiler. He has the great merit of having first made known, beyond the precincts of Germany, the admirable collection of facts contained in the numerous medico-legal periodicals of that country. He has also had recourse to the numerous French authorities for many of the most valuable facts of toxicological science, particularly in the physiological and chemical departments. But the use which our author has made of British authorities will, perhaps, in the eyes of some readers, be even more valuable than all that he has collected from foreign sources. "Our own language abounds in interesting facts relative to the medical jurisprudence of poisons; but the information has hitherto lain scattered in such a multitude of publications, and these of such a multifarious nature, that they are now first rendered available, and made to contribute to the extension of the knowledge of medical jurisprudence."

We cannot too highly extol the pains which Dr Christison has taken in the quotations to refer with extreme accuracy to the original authorities. This precaution, which is proper on all occasions, is absolutely necessary in a medico-legal work; for in Scotland a witness is allowed to support his opinion by express reference to published authorities; and we have no doubt that Dr Christison's work will immediately be sanctioned in our courts as a standard authority, and that the advantages which the administration of justice will derive from it, will assist in superseding the present absurd banishment of authorities from the English system of medical evidence.

The work itself is divided into two parts,—the one treating of General Poisoning, and the other of Individual Poisons. This arrangement possesses great advantages in the consideration of a subject which comprehends so many particulars as belong to Toxicology. The immense multitude of facts connected with individual poisons which have been already collected, and are daily accumulating, often apparently inconsistent and contradictory, rather confounds the judgment and oppresses the memory, until by severe criticism of particulars and strict induction, we arrive at those general doctrines, which reduce the whole to order, and serve as a sure direction in our further prosecution of the subject. They, in fact, constitute the science of Toxicology; and to one who is thoroughly conversant with them, the acquisition of the knowledge of individual poisons presents no difficulty. The detailed history of each may be considered as an appendix,—a mere collection of authorities.

Dr Christison divides the first part of his work, which is in a great measure original, into three chapters, treating of the physiological action of poisons; of the evidence of general poisoning; and of imaginary, pretended, and imputed poisoning.

The inquiry into the mode of action of poisons, and of the causes which modify it, is of great importance to the physiologist as well as to the toxicologist, for it comprehends points of the highest interest regarding the phenomena of life, as elucidated by the operation upon them of the most powerful and easily ascertained external agents.

Dr Christison, in proceeding to investigate the mode of action of poisons, considers it as being local or remote. The local action is either corrosive, irritating, or consists in a peculiar impression on the sentient extremities of the nerves. The two first modes of action require no illustration. The third is not so generally admitted, and not so easily established. Yet it is necessary for the stability of the very generally received doctrine of the transmission of the influence of poisons from organ to organ along the nerves. The facts collected by Dr Christison put this mode of local action out of doubt. In one of these three ways, or in two or all of them united, the direct and immediate influence of all poisons takes place. This primary impression sometimes declines and passes away without producing any effect upon the system in general; but sometimes atrophy and death follow from the local disorganization of the stomach produced by the swallowing of caustic poison, such as nitric acid.

More generally the action of poisonous substances extends from the part to which they are applied to other organs; and we perfectly agree with our author in preferring the term *remote* to that of *general*, to designate these effects; because "the latter implies an action on the general system or whole body; whereas it appears probable that an action of such a kind is rare, and that most poisons which have an indirect action exert it on one or more of the important organs only, and not on the general system."—P. 4. This doctrine, which is fully established by the experiments of recent physiologists, we may remark, is conclusive against that generalization of the whole vital powers under the term of Excitability, as one and indivisable, which for a long period was considered as one of the happiest conclusions to which the human intellect had arrived in reference to medical science.

There are only two ways in which pathologists have conceived that the agency of poisons can be conveyed from organ to organ; the one is by the medium of the nerves, called sympathy,—and the other by means of the vascular system, by absorption. At different periods each of these methods of transmission has been generally received to the exclusion of the other, but it is almost certain that sometimes the one, sometimes the other, is the real agent; and it is even probable that sometimes both are at different times called into action. We are quite aware that this doctrine is at variance with a dogma often quoted as sound philosophy, and recently in regard to poisons, that it is contrary to an established law of nature to accomplish the same end in the animal economy by two different means. On the contrary, we think we should have no difficulty in showing, that on many occasions nature does not trust her important functions in the animal economy to a single means, but is possessed of secondary means to serve as substitutes when the principal one fails. Concentrated poisons are believed to act more frequently on the nervous extremities, diluted poisons to be more frequently absorbed.

The existence of sympathetic extension of action is most satisfactorily proved where there is no material agent to be absorbed, as in the sneezing excited by a strong light falling upon the mucous membrane of the nostrils, the nausea induced by disgusting sights, besides the effects of mechanical injury and natural disease adduced by our author. The communication by sympathy being admitted, it next becomes a matter of curious inquiry, by what tests or characters we can ascertain whether a given poison acts by sympathy. Our author seems to admit it in those cases in which the influence of the poison takes place before, in all probability, its particles could be conveyed to the sympathizing organ by the circulating fluid. Thus, in experiments made at Leicester, dogs were killed in eight, seven, and even three seconds by hydrocyanic acid, but this time was quite insufficient for any portion of the poison being conveyed from the stomach to the organs acted upon, through the medium of the circulation. The identity of symptoms succeeding the administration of a poison with those induced by mechanical injury or natural disease, forms also a strong presumptive proof that its action is by sympathy. Lastly, Dr Christison has brought forward a very ingenious experimental trial, which appears to us to be in particular cases quite conclusive. By diluting any powerful agent it is more adapted for absorption, and, in fact, it promotes the action of some, but of others it destroys the deleterious impression altogether.

After the numerous experiments of Magendie, Emmert, and others, the transmission of poisonous agency by absorption seemed to be indisputably proved, yet the obvious conclusion has recently been controverted with great ingenuity and ability by Mr Morgan and Dr Addison. We have not room to detail these experiments sufficiently, and shall content ourselves with the conclusion at which they arrive, and which may be considered as the most modern and perfect modification of the doctrine of action exclusively through sympathy.

> "In order to explain the continuance of the action of poisons when, as in Magendie's experiment, the part directly poisoned communicates with the trunk by blood-vessels only, or when, as in their own experiment, the poison is introduced into a vein, but prevented from reaching the heart,—they suppose, that, like the other membranous cavities of the body, the inner surface of the vascular system is supplied with an expansion of nervous filaments, on which poisons produce their peculiar impressions, and from which these impressions are communicated along the nerves to remote organs. In order, however, to account also for the correspondence observed between the activity of poisons and the activity of absorption in the several textures to which they are applied, it would be further necessary to maintain, that the nervous expansion of the inner membrane of the blood-vessels is more peculiarly fitted, than the sentient extremities of the nerves elsewhere, for receiving the impressions made by poisonous agents;—nay perhaps, that it is the only nervous expansion which possesses that function, except in regard to those poisons which cause evident organic injury, such as inflammation or corrosion. This important doctrine is one towards which Mr Morgan and Dr Addison evidently lean, although they have not in their work adopted it explicitly."—P. 12.

When quoting this passage, which ascribes the powerful operation of poisons admitted into the blood-vessels to their impression on the nervous filaments distributed on their lining membrane, we were forcibly reminded that Dr Cullen expressed his belief that the internal surface of the blood-vessels had very little sensibility, and was therefore little liable to be affected with weak impressions, for the very purpose of using it as an argument against the theory of absorption. "We consider the operation of stimulants taken in by the mouth to be in the blood-vessels very little, and know of no observation, or experiment that leads us to think otherwise." (Cullen's Mat. Med. Vol. ii. p. 143.) So easily does preconceived opinion invent supposititious facts.

We agree entirely with Dr Christison, that the general principle that absorption performs a very material part in the operation of poisons, still remains uncontradicted.

In reference to the absorption of poisons into the blood, it is now quite established that certain poisons after being swallowed have been detected in the blood, secretions and excretions; and Dr Christison gives a variety of reasons why, in general, poisons which seem to enter the blood, are not to be detected either in that fluid or in the solids. As this sometimes depends on the minute quantity which enters the blood, it should form an inducement to the toxicologist to perfect his processes of analysis, so that those which now escape his analytic skill shall also be made manifest.

Dr Christison next proceeds to consider the organs affected by the remote action of poisons. In some cases they exert their influence on one organ almost exclusively; in others, they operate upon several nearly equally. The purest examples of poisons which do not enter the blood produce symptoms of depressed heart. Of the poisons which act remotely, either from absorption or nervous communication, many act on the heart almost solely, others on the lungs, brain, or spinal cord, and of some, as oxalic acid and arsenic, the action is multifarious.

We have next a very satisfactory inquiry into the causes by which the action of poisons is modified;—their quantity, state of aggregation, chemical combination, mixture, difference of tissue, difference of organ, and lastly, habit and idiosyncrasy. Under all of these heads many curious remarks occur.

This branch of the subject is concluded by the application of the foregoing remarks to the treatment of poisoning in general. On the subject of chemical antidotes, we find a remark deserving of being universally known; that, if poisons are capable of remote action, mere neutralization of their chemical properties is not sufficient, for it appears that such poisons act throughout all their chemical combinations that are soluble. It is therefore necessary that the chemical antidote render the poison insoluble or nearly so.

In the instance of external poisoning, the grand object of the practitioner is to prevent the poison from entering the blood, or to remove it from the local vessels into which it has entered. The method revived by Dr Barry, of the application of cupping-glasses, sucks the blood out of the wound, and consequently washes away the poison with it; and Dr Christison conjectures that it also compresses the nerves, and prevents the impression made by the poison on their sentient extremities from being transmitted along their filaments. The simple application of a ligature between the injured part and the trunk will always retard the operation of the poison; and, as poisons of an organic nature are in no long time decomposed in the blood, they may, by means of a ligature, be admitted into the system in successive quantities, each too small to do serious mischief. The combination of ligature with venesection deduced from the experimental researches of M. Verniere is exceedingly ingenious. A ligature is to be tied so as to compress the veins without compressing the arteries of a limb, and a vein is to be opened between the wound and the ligature, so that the blood which flows out must previously pass through the wound and carry with it the poison. M. Verniere previously induced venous plethora to retard absorption, but it is probably unnecessary, and renders the operation more difficult.

The second chapter, on the evidence of general poisoning, is purely medico-legal, and is one of the greatest importance. But it scarcely admits of abridgement. It comprehends an account of the various kinds of evidence by which the medical jurist is able to pronounce whether poisoning, without reference to a particular poison, is impossible, possible, probable, or certain. This evidence is derived from five sources: 1*st*, the symptoms during life; 2*d*, the appearances in the dead body; 3*d*, the chemical analysis; 4*th*, experiments and observations on animals; and 5*th*, certain moral circumstances, which are either inseparably interwoven with the medical proof, or cannot be accurately appreciated without medical knowledge. The value of the evidence derived from each of these sources is very accurately estimated; and although the author has always kept in view, that in many cases the chief object is to establish the impossibility of poisoning, and to silence vague rumours and conjectures, yet he also controverts the opinion expressed by all German and most French authors in medical jurisprudence, that poison can never be completely substantiated unless the particular poison be found out; for in some cases the probability from medical evidence may run so high, that, in conjunction with other circumstances of general evidence, no rational being can entertain a doubt that poisoning has been perpetrated. Indeed, in this country, convictions have recently taken place where no satisfactory proof existed what poison had been given. We need not, however, say that in such cases the utmost caution is requisite.

In regard to the evidence of poisoning derived from the symptoms, Dr Christison thinks, that, in abandoning the old method of determining questions of poisoning from the symptoms alone, medical jurists have gone too far in laying down, that the symptoms, however highly developed, can never justify an opinion in favour of more than high probability. He thinks they have injudiciously confounded together actual symptoms with their general characteristics. The chief characteristics of the symptoms of poisoning are, that they commence suddenly, and prove rapidly fatal,—that they increase steadily,—that they are uniform in nature throughout their course,—that they begin soon after a meal,—and that they appear while the body is in a state of perfect health.

We cannot enter into the elucidations of all these points given by our author, nor into the inquiry how far these characteristics distinguish the symptoms of poisoning from those of natural disease. It will be sufficient to quote the very sound conclusion to which our author arrives.

> " On considering all that has now been said regarding the characteristics of the symptoms of general poisoning, as contrasted with those of natural disease, no one can hesitate to allow, that from them alone a medical jurist can never be entitled to pronounce that poisoning was certain. At the same time he must not on that account neglect them. For, in the first place, they are of great value as generally giving him the first hints of the cause of mischief, and so leading him to search in time for better evidence. Next, they will often enable him to say that poisoning was possible, probable, or highly probable; which, when the moral evidence is very strong, may be quite enough to decide the case. Thirdly, although they can never enable him to say that poisoning was certain, they will sometimes entitle him to say on the contrary, that it was impossible. And to conclude, when the chemical or moral evidence proves that poison was given, the characters of the symptoms may be necessary to determine whether it was the cause of death."—P. 42.

With regard to the evidence from morbid appearances, Dr Christison is of opinion, that, except in the instance of a very few poisons, the morbid appearances alone can never distinguish death by poison from the effects of natural death, or from some other kinds of violent death. They may suggest that poison was the cause of death, or that the death was natural, and yet in both cases the conclusion may be wrong. Therefore, " the medical inspector in a question of poisoning, must take care not to be hurried away by the first striking observation he makes during an inquiry, and so be induced to conduct the rest of the inspection superficially; and likewise, that he should not so frame his opinion on the case, as to exclude the possibility of a different cause from the apparent one, unless the appearances are such as must necessarily have been the cause of death."—P. 46.

The evidence derived from chemical analysis is generally and correctly considered as the most certain. But in some cases corroboration is necessary. Granting that the poison be found, a comparison with the symptoms may be necessary to prove that it was the real cause of death. Dr Christison has quoted two striking cases in illustration, one of which, reported by Dr Wildberg of Rostock, we shall quote. "Wildberg was required to examine the body of a girl, who died while her father was in the act of chastising her severely for stealing, and who was believed by all the bye-standers, and by the father himself, to have died of the beating. Accordingly, Wildberg found the marks of many stripes on the arms, shoulders, and back, and under some of the marks blood was extravasated in considerable quantity. But these injuries, though severe, did not appear to him adequate to account for death. He therefore proceeded to examine the cavities; and on opening the stomach, he found it very much inflamed, and lined with a white powder which proved on analysis to be arsenic. It turned out, that on the theft being detected the girl had taken arsenic for fear of her father's anger, that she vomited during the flogging, and died in slight convulsions. Consequently, Wildberg very properly imputed death to the arsenic. In this case the chemical evidence proved that poison had been taken; but an account of the symptoms and appearances was necessary to prove that she died of it."—P. 48.

Sometimes even when it is certain that poison, and of a known species, has been swallowed, none of it may be detected. It may have been all discharged by vomiting or purging, or it may have been absorbed, or it may have been decomposed. Because poison is not found, we cannot from that alone conclude that it has not been the cause of death.

Experiments upon animals with the remains of the suspected article, or the matters from the stomach, are neither so conclusive as they were once thought to be, nor altogether valueless.

The moral evidence of poisoning is derived from a variety of circumstances, in the investigation of which a professional witness possesses some peculiar advantages. The points to which his attention should be directed, and the line of conduct which he should follow, are here detailed with accuracy and acuteness.

The portion of Dr Christison's work allotted to General Poisoning is concluded by a short chapter on imaginary, pretended, and imputed poisoning. The first is a variety of mental derangement, and commonly betrays itself in other parts of the conduct of the individual, and of the other two very good examples are inserted by Dr Christison, the one of which came under his own notice, and the other occurred to Mr Thackrah, and was tried at the York Assizes,—but they are two long for quotation.

From this imperfect analysis of Dr Christison's observations on General Poisoning, our readers will be able to form an imperfect idea of the excellence both as to matter and manner of this preliminary and abstract part of his classical work. Our account of the second part, treating of Individual poisons, we must necessarily postpone to another occasion.

Art. X.—*Elements of Practical Chemistry, comprising a Series of Experiments in every department of Chemistry, with directions for performing them, and for the preparation and application of the most important tests and reagents.* By David Boswell Reid, Experimental Assistant to Professor Hope, &c. &c. Maclachlan and Stewart, Edinburgh. 1830.

A valuable improvement in the mode of teaching chemistry has of late years taken place in this city, in the institution of classes for the practical cultivation of the art, and the performance of experiments either to illustrate general doctrines, or to demonstrate particular facts. By availing himself of this mode of instruction, the chemical student, who previously had to satisfy himself with hearing the lectures, and witnessing the experiments of the teacher, or, if he was desirous of more intimate acquaintance, had to obtain it in a very imperfect, and often expensive and dangerous manner, by repeating by himself the experiments exhibited in public, may now, in a very short time, become familiar with all the most important facts of which chemical science at present consists. In this manner he not only acquires habitual dexterity in experimenting, but the practice of observing forcibly impresses on his mind the nature of chemical action, the effects of chemical processes, and the laws by which they are regulated. While the student, in short, obtains by public instruction a general knowledge of chemical doctrines, in the laboratory he becomes practically familiar with chemical phenomena, and the circumstances on which their action depends.

It is not difficult to perceive that the systematic work which is intended as a companion to lectures, is by no means calculated to serve as a guide in the laboratory. In the former, the principal object is to explain the general principles of the science as already established from ascertained facts, and to deliver the chemical history of the material world, illustrated occasionally by experimental proofs. All minute detail must be avoided as tending rather to withdraw attention from the leading and characteristic features of the science. In the latter, the student who is supposed to be already impressed with the general principles, begins to study such processes as, though individual, tend to establish general rules, and to become familiar with the details of chemistry as an experimental art. In so doing, he is compelled to attend not only to the chemical qualities of the objects on which he operates, but to the proportions in which their mutual action is observed to take place, and to the results which respectively arise from their combination.

Though the library of the practical chemist has been improved by the addition of several works by Accum, Griffin, and particularly the excellent treatise of Mr Faraday, we have no one which is calculated exclusively as a guide to the operations of the laboratory. With the view of supplying a system of the requisite instructions for such a course of study, Mr Boswell Reid, who is already known as the author of a short treatise on chemistry, has composed the present work; and it is but just to say, that, by bringing to the task not only the requisite chemical knowledge but the advantage of experience in the mode of conducting practical courses, he has rendered it in every respect suitable to the object in view.

It consists of two parts;—the first an arranged series of useful experiments, illustrating the general and particular laws of chemical action, and the chemical characters and relations of the most important objects of the material world; the second embracing the consideration of a variety of miscellaneous subjects, relating to apparatus, modes of experimenting, the use of the blowpipe, the employment of electricity and galvanism, and every means which the chemist employs in the course of his investigations. In the whole work Mr Reid shows not only thorough acquaintance with every part of his subject, but a good deal of facility in the manner of explaining the different processes. To show how much order and precision he observes, it is merely requisite to say, that in all the experiments the proportions of the different substances are stated according to the principle of chemical equivalents, or, in other words, upon the grounds now well-established, that bodies combine in certain definite proportions. The changes effected by chemical action he illustrates very ingeniously by the use of diagrams, expressing the proportions in which the different ingredients exist before and after decomposition. To facilitate the mode of experimenting he has also illustrated many of his experiments by wooden cuts of the apparatus employed. In short, though it is impossible to specify all the merits of this work in the compass of a short notice like the present, we may say in general, that it is extremely well calculated for the purpose for which it is designed; and, while it does great credit to the ingenious author, it promises to be extremely useful to the student who is desirous to possess more than speculative knowledge of the interesting science of which it treats.

PART III.

MEDICAL INTELLIGENCE.

MORBID ANATOMY.

Case of General Melanosis, [*Répertoire Général d'Anatomie et de Physiologie Pathologiques*, 1829, *Premier Trimestre.*]—Professor Lobstein of Strasburg describes in this Journal the following interesting case, where the deposition of melanosis took place throughout a great variety of the tissues of the body. A lady, fifty-four years of age, remarked six years before her death a black spot on the inside of the left leg, at its articulation with the thigh. Gradually the spot became a black wart, to remove which various attempts were made by means of ligatures and various caustics, but which, notwithstanding all the remedies resorted to, continued slowly to increase till in 1827 it attained the size of a hen's egg. It then softened, the skin over it was abraded, fungoid excrescences formed on its surface, a sanious matter was discharged, and around it arose many small hard tumours, some of them flesh-coloured, and others bluish, but all indolent and insensible. By little and little the disease spread on the leg and thigh; and on the upper and inner part of the latter a very prominent tumour was produced. The liver also became large, and a projection or bump could be felt on its convex surface. Towards the end of her life a great number of little tumours of the same colour and hardness with the rest were discovered in the scalp, and one arose in the upper eyelid of the left eye, near its inner angle. Meanwhile the patient became more and more feeble and cachectic, without precisely having fever. The discharge from the principal tumour was fetid, and so great, that in less than an hour it soaked through twelve folds of cloth. Latterly the prostration of strength was such as to render the lady unable to stand. In the three last days of her life there was no pulsation to be felt in the wrists; the voice was lost; and death was preceded by a desire to vomit and eructations. She retained her faculties to the very last.

The original tumour rested on the ligamentous bands and periosteum of the parts forming the joint; it consisted of fungoid, reddish lumps, agglomerated, and separated from one another by fissures; and it had attained a size considerably greater than that of an egg. The tumours around it were like large warts, reddish-yellow in colour, and as hard as cartilage. They were so numerous, that on a piece of skin, measuring three inches by two inches and a half, Professor Lobstein counted sixty-eight, varying from a line to nine lines in diameter. On a portion of skin from the thigh, measuring six inches by two and a half, he found forty-three small medullary tumours, of a flesh-red or bluish colour, of the hardness of cartilage, and all filled with a black matter. They were situated in the true skin and cellular tissue, never penetrated the aponeurosis, and were not surrounded by any sign of inflammation. At the upper and inner part of the left thigh, near where the saphena joins the crural vein there was a mass of six encephaloid tumours, the largest of which was two inches and a half long by eleven lines in breadth, and all of them were softened. The melanotic matter abounded in them all. The vessels behind this mass were not altered in appearance, except that the coats of the saphena vein were as thick as those of an artery. The lungs were crowded with little hard tumours as big as lentils, without their parenchymatous structure being diseased. The liver opposite the gall-bladder presented on its convex surface a tumour of the size of a small apricot, which contained a homogeneous matter as black as China ink, deposited in a soft friable tissue. The spleen contained two tumours, one of them eleven, the other seven lines in diameter, and consisting of mixed yellow, gray and black matter. The right kidney was healthy in structure, but presented at its upper extremity a tumour three inches and a half long, eight inches broad, and an inch and three quarters in thickness, which at first appeared a semitransparent hydatid, but which proved to be a cyst divided into several cells, filled with a black pulpy mass, and having a free communication with the renal vein. The great veins of the pelvis were filled with a polypous blood adhering to their inner surface. When macerated several days, these concretions became fibrine, which in some parts presented a regular stratified appearance.

PHYSIOLOGY AND PATHOLOGY.

On the Aromatic Principle of the Blood, and the differences it presents in different Animals and different Sexes. (Annales d'Hygiène Publique et de Médecine-Légale, Avril 1829.*)* In the number of this Journal for January 1828, some notice was taken of the mode of determining by chemical analysis whether stains on clothes, knives, or other objects suspected to have been produced by blood are really such or proceed from other causes. The tests which were described on that occasion, and which were ascertained by Professor Orfila, it now appears may be applied successfully after a lapse of months or even years. M. Barruel, however, advances a step farther, and maintains that, by means of an aromatic principle, which he conceives exists in the blood, the chemist may distinguish whether blood has proceeded from the human subject or from one of the lower animals,—what the animal is from which it has proceeded,—and whether in the case of human blood, it is that of a man or a woman. He says that some years ago, when he was assisting M. Vauquelin in some of his experiments, he remarked, that when sulphuric acid was poured on the crassamentum of ox's blood, a strong odour of a cow-house was exhaled. More lately in analyzing the blood of a man who had poisoned himself with opium, he also remarked, that when sulphuric acid was heated in a matrass to ebullition with the blood, an odour of the sweat of the human male was discharged, of such strength, as to compel him to quit the apartment. This circumstance having brought the former fact to his recollection, he proceeded to make further experiments on the subject, and obtained the following results.

The blood of every animal contains a peculiar aromatic principle, which has the same odour with its sweat and pulmonary exhalation. This principle, while it exists in combination with the blood, has no odour; but when the state of combination is broken it is volatilized, and then the species of animal may be recognized by the odour evolved. The principle has the most powerful odour in the male of each species. It may be developed either from the entire blood, or after the fibrin has been separated, or from the serosity. The best mode of developing the odour is to pour a few drops of blood into a glass, to add between the third part and the half of its volume of concentrated sulphuric acid, and to stir the mixture with a glass rod; upon which the odour will at once be disengaged. By this simple method M. Barruel detected in the blood of man the strong smell of the male human sweat, such as could be confounded with nothing else; in the blood of a woman he found the same odour, but much more feeble; in that of the ox a strong odour of a cow-stable or cow-dung; in that of the horse a strong odour of horse's sweat or horse-dung; in that of the ewe a distinct odour of wool impregnated with its oil; in that of the ram an analogous odour mixed with a strong odour of the goat; in that of the dog the odour of its transpiration; in that of the pig a disagreeable smell of a pig-stye; and in the blood of the rat the disagreeable smell of that animal. Analogous results were obtained with the blood of fowls, turkeys, ducks, and pigeons; the blood of a frog gave out a distinct smell of marsh rushes; and that of the carp an odour resembling the smell of the slime which covers fresh water fishes. He farther found, that even after blood had dried on cloth, and remained there for fifteen days, the aromatic principle might be disengaged by moistening the stained portion of the cloth and subjecting it in a glass to the action of sulphuric acid.

The experiments of M. Barruel have been repeated by various chemists in France, some of whom were unable to procure the results at which he arrived, while others procured analogous results as to many points, though not in every department of the subject. It appears, for example, that the odour exhaled by the blood of men is sometimes not distinguishable from that of women; for it is always strongest in people who have dark hair, so that the aromatic principle of the blood in a dark-haired female, and that of a light-haired male do not differ materially. M. Soubeiran, who makes this correction on M. Barruel's conclusions, adds, that he could not always satisfy himself that the smell of the aromatic principle of the blood of different animals was specific and characteristic of the animal. M. Raspail denies the accuracy of M. Barruel's statements altogether. M. Villermé was equally unsuccessful with M. Raspail. M. Leuret goes along with M. Barruel in almost all his propositions, and M. Chevallier seems to have obtained results equally concordant. A remarkable fact is stated by M. Leuret. He sent to M. Barruel the blood of a man, of a woman, of a horse, and of an ox, contained in numbered phials; and M. Barruel distinguished among them the blood of the two last, and pronounced that both the two first were human blood; but he inferred, from his experiments, that the male blood was that of a woman, and the female blood that of a man. This error was afterwards easily explained: The woman from whom the blood was taken was dark and of a strong frame, while the man was of a lymphatic temperament, with delicate skin, and no hair on the face. These criticisms on M. Barruel's discovery are contained in the Annales d'Hygiène, &c. for Ocotber last, and in the Revue Médicale for September. It is added, in explanation of the success of M. Barruel, and the failure of others, that this gentleman has a peculiarly delicate sense of smell. The whole subject is well worthy of farther investigation, as promising to supply the medical jurist with a very useful instrument of research in judicial cases.

Case of Vagitus Uterinus. (In a Letter to Dr Duncan from Dr A. F. Holmes, Lecturer on Chemistry and Materia Medica, Macgill College, Montreal.)—Having lately met with some discussions on the possibility of the fœtus emitting cries in utero, I am induced to send the following case, and to request, if it seems to have any thing curious in it, that it may be inserted in the Edinburgh Medical and Surgical Journal.—On 29th November 1828, I was called to a lady in labour of her sixth child. The fontanelle presented, but the pelvis being capacious, and her labours generally easy, no attempt was made to change the position. The head continuing to descend, the mouth lay on the pubis, and the examining finger could easily be introduced into it. The occiput did not yet occupy fully the cavity of the sacrum. At this time I heard sounds like the cries of a child whose mouth was muffled by some covering, but not being very distinct, and not being at all prepared for them, I thought, when they ceased, that they must have been produced by flatus in the intestines of the mother. In the course of a short time, however, the cries were repeated, and with the greatest distinctness, so as not to admit of a doubt that they proceeded from the child. The mother much alarmed, inquired the cause of these noises, and required to be assured they were not indicative of any danger. The pains being brisk, the head was soon forced down and expelled. The child was a female, and is still alive and thriving. This case appears to me so curious, though easy of explanation, when the position of the mouth is considered, that I am induced to draw up this notice, not having met with any thing similar on record, and as it is entirely different from the incredible stories we have of the fœtus emitting cries before the commencement of labour.—*Montreal, August* 26, 1829.

White or Milk-like Serum. By HEWSON and Mr ANDERSON of Dumfries.—"Although the serum of human blood," says Mr Hewson, "be naturally transparent, and a little yellowish, yet it is frequently found to have the appearance of whey, and sometimes to have white streaks swimming on its surface like cream, and now and then to be as white as milk, whilst the coagulum is as red as usual. In all these three cases of whiteness, I have examined it in a microscope with a pretty large magnifier, and have found it to contain a number of very small globules, although naturally, when transparent, no globules can be observed in it, notwithstanding what has been affirmed by some authors. These globules differ from the red particles (improperly called globules) in their size, which is much smaller; and likewise in their shape, which is spherical, whilst the red particles are flat. They agree more with the globules of milk. I have compared them with those of woman's milk, and have found, that in the milk the globules are of different sizes, some being three or four times as large as others, and the smallest little more than just visible, when viewed with a lens of one-twenty-third of an inch focus, whilst those of the white serum are more regular, and are all of them about the size of the smallest globules of milk. Of this white serum I have met with the following instances in books. In Tulpius, (Ob. l. 1. cap. 58.) one instance; in Morgagni, (Ep. xlix. Art. 22.) two; in the Philosophical Transactions, (No. 100 and 442.) some instances; in Sckenckius's Observations, (Obs. lib. 3.) two cases are related from other authors. I have likewise heard of the same appearance having been observed by the learned Sir John Pringle, Dr Pitcairn, Dr Hunter, Dr Watson, Dr Bromfield, Dr Garthshore, and Dr Fothergil of Northampton. And other instances have lately occurred to persons of my acquaintance, who have favoured me with a short account of them."

The first of these cases occurred in a woman of twenty-five, labouring under *suppressio mensium* for seven months, discharging blood sometimes by vomiting, sometimes by stool, with pain in the left side and stomach, nausea, lassitude and sleepiness, and a pulse at 95. For these complaints she lost blood twelve times in the course of six months, and each time the serum was as white as milk.

In the second case, a man of thirty-five, corpulent, subject to bleeding at the nose, and piles, with night sweats, was, after their cessation, attacked by headach for two or three days, followed by epistaxis to the amount of about two pounds. The blood thus discharged was as white as milk. At ten the same evening the hemorrhage returned to a considerable extent; and it was then thought proper to bleed him, which was done to the amount of sixteen ounces, with the effect of producing fainting and cessation of the hemorrhage. The *serum* of this blood was also very white. Two days after, the headach recurred, and the nose began again to discharge blood, the serum of which was no whiter than whey; and the bleeding continued during most of the night to the extent it was supposed of two or three pounds, the serum, though still white, being so much less so that the bottom of the vessel could be seen through it. The bleeding returned repeatedly during the space of eight days, when it stopped spontaneously, the serum having become more transparent towards the last.

In the third case, it occurred in the person of a butcher, tall, of strong make, a hard drinker, who was subject to vomit every morning, took little food; and, though he sweated much, did not waste in flesh. He was bled for slight *asthma*, to which he was subject, and which was always relieved by blood-letting.

In a fourth case, it occurred in a person of full make, but rather muscular than corpulent, labouring under violent rheumatic pain in the hip, for which he was blooded three times; and each time the *serum* was as white as milk, but the coagulum of its natural colour.

The cases, of which these short abstracts are given, form a sort of introduction to the following one, which has fallen under the observation of Mr William Anderson of Dumfries.

" A man attached to one of those small shows which travel the country, applied to me about ten o'clock in the forenoon (October 1827) complaining of severe pain in the region of the heart, which he ascribed to loud and continued shouting the previous day; pulse small, and rather quicker than natural, accompanied with difficult respiration. I abstracted about a pound of blood from the arm with the effect of relieving him; and before the bleeding was finished, I observed the blood assume a bluish appearance, and was surprised, after it had coagulated as usual, to observe, instead of serum, the coagulum floating in pure *milk*. The man then went to his lodgings, and I was called to see him in about an hour after the first bleeding. The pain had returned in a greater degree than before. A second bleeding again relieved him; and the blood assumed the same appearance as before, and separated in the same manner as after the first bleeding. I inquired if he had been drinking any milk in the morning; he replied, that he had been drinking a considerable quantity about three hours before he applied to me. This man was in good health before this attack from over-exertion, and was completely relieved by the second bleeding. The pain did not return, and he got well and continued so during his stay here.

" I have conversed with several medical gentlemen on this case, none of whom ever met with, or remember of having read of such a case. I am not aware of any organic derangement that would admit of fluids being received into the circulation in the same state in which they are taken. If fluids enter the circulation in she same state in which they are taken, as this case seems to demonstrate, our present theory of absorption must be wrong; we being led to believe, that, whatever is taken as food, must undergo a certain change before it can be received into the blood-vessels through the absorbents.

" *Milk*, of all other fluids, being an animal secretion, is most likely to enter the circulation without requiring any change from the digestive organs, but from this case I am inclined to believe, that other fluids may enter in a similar manner. In this case the milk seems to have entered the circulation in a very short time, between two and three hours at most, scarcely so long as anatomists allow for digestion alone.

"The milk in the above case was not analyzed, but its appearance and consistence were the same as other milk. It remained fluid until it was thrown out, which was several days after it was abstracted. The red part of the blood distinctly coagulated and floating in the milk; and what serum might be mixed with it did not diminish its body of colour as water mixed with milk would do. While the blood was flowing it had the same appearance as other blood, and it was only when it began to cool that I first observed the difference. If the person in this case had not been drinking milk a few hours before he was bled, and the same appearances had presented themselves, I should have been at a loss to conjecture what might have caused them; but the rarity of the occurrence, and the bleeding following so soon after having taken the milk, induces me to believe that this alone was the cause; for what else could produce such appearances?"

The cases above narrated show that milky serum is not so rare as Mr Anderson seems to imagine. We may further add, that we have several times seen opaque milky serum in persons whom we have had occasion to bleed for general dropsy; and several of our professional friends have met with almost every degree of opacity up to the milky appearance remarked by Hewson and Mr Anderson.

The explanation of this state of the blood is not so easily accomplished. Hewson appears to have at one time thought it depended on the recent introduction of chyle into the blood, before complete hæmatosis.

"When I first saw this unusual colour of the serum," he observes, "I was inclined to adopt the opinion of those who have attempted to explain it by the patient's being bled soon after a meal, or before the chyle was converted into blood. But afterwards, on considering the cases above related, I found this could by no means be the cause, as none of these patients had taken a sufficient quantity of food to occasion this appearance; on the contrary, most of them had a bad appetite, and had taken remarkably little food, and were subject to vomitings. I therefore concluded it was owing to something else, and what confirmed me in this opinion, was an observation I had repeatedly made in dissecting geese, whose serum I had frequently seen white, whilst their chyle was transparent; although they had been killed only three or four hours after eating. And as the whiteness, in all the cases that I examined, was owing to a quantity of small globules like those of milk, (which are known to be oily,) I concluded that these in the human serum, when white, were oily likewise, and recollecting to have read somewhere of an experiment by which butter had been got from such human serum, I tried, by agitating some of it a little diluted, to separate its oil, or to churn it, but without success. I then inspissated some of it to dryness, and compared it with the natural serum of human blood prepared in the same way, and found it less tenacious, and much more inflammable; and when thus dried, its oil oozed out so much as to make the paper in which it was kept greasy. Another portion of this white serum being kept some days, putrified, and, when putrid, it jellied as milk does when become sour; but it differed from milk, in being extremely fœtid."

He does not, however, positively infer that the recent introduction of chyle may not occasionally colour the serum; but if this were the uniform cause, he remarks, we ought to see it oftener. The idea that it is occasioned by the absorption of fat, is adopted by the late Dr Gregory, who used to mention the circumstance in his preliminary lectures, and by whom it is also stated in his *Conspectus*. "Serum aliquando *album*, *instar lactis*, observatur, quod ab *adipe resumpto*, neque iterum in gluten redacto oriri videtur." That it depends on the presence of oil is rendered probable by the observation of Dr Traill in a former number, and by some remarks which will appear in our next.

PRACTICE OF PHYSIC.

Cases of Ileus cured by Quicksilver. (*Journal der Praktischen Heilkunde, Mai et Junius* 1829.)—The old method of treating ileus by large doses of running mercury has long ago fallen into disrepute, and although examples of its successful administration have been published from time to time even in late years, it has not been regarded with any confidence by physicians for a long period. The latest writer on the disease, *Dr Abercrombie*, discusses the therapeutic effects of mercury in three lines, stating that he has repeatedly tried it in doses of one or two pounds, and never saw it produce any other good effect except some diminution of the vomiting. *Dr Ebers* of Breslau, who, in the numbers of Hufeland's Journal quoted above, has written a long and learned paper on its employment, is of opinion that when used in quantities sufficiently large it will more frequently prove efficacious than is generally imagined, and, on the whole, conceives that it is a remedy, which, though far from certain in its operation, has been a great deal too much neglected in modern times.

He observes that he has succeeded in curing three very bad cases of ileus with it. Of one of these he unfortunately lost his notes. But the two others are related in his paper at great length. The following is a summary of them: *Case First.* A little, weak female, forty-four years of age, and the mother of many children, had been for some years subject to fits of colic, which she usually subdued by spirituous remedies. One of her customary fits attacked her on the 19th September. Next day, notwithstanding the employment of her often-tried remedy, the colic was worse, and was attended with constipation. On the night of the 20th the pain was so excruciating that she cried like a mad woman. When Dr Ebers saw her at five on the morning of the 21st, she complained every two or three minutes of paroxysms of deep-seated violent pain in the hypogastrium, extending over the whole belly, but affecting most acutely the right iliac region, where also there was more tenderness or pressure than any where else. She tossed about in bed, and cried aloud. The heat of skin was little increased, the pulse was small and hard, the tongue natural, the breathing quick and anxious, the urine scanty, and the bowels constipated since the 19th. The patient vomited every thing she swallowed. There was no appearance of a hernia. Dr Ebers being disposed to consider her complaint an inflammatory colic, directed twelve ounces of blood to be drawn, and ordered first a combination of anodyne and laxative remedies, with a clyster, then sixteen leeches, then more laxatives, then leeches again; next another blood-letting, and again clysters and an anodyne emulsion,—all in the course of the 21st. Next morning he found her exhausted by her sufferings, but the fits of colic as violent as ever. She threw herself from one part of the bed to the other, cried out for help, and had an expression of most acute suffering. The pulse was sunk. The pain of abdomen felt on pressure bore no proportion to the pain complained of as accompanying the spasmodic fits. No fæces has been passed by the anus; but they had begun to be discharged by vomiting, which was incessant. Leeches were re-applied, emollient clysters injected, and anodyne emulsions with cathartics administered, but all without relief. A little laudanum *(ten drops!)* was tried, then croton-oil with the senna electuary, but still without avail. At night the abdomen had enlarged greatly, was more tender, and gave an impression of doughiness when handled. Croton-oil and calomel were ordered, and frictions with croton-oil and hyoscyamus, also fomentations, and oleaginous clysters. The oil of the clysters was discharged by vomiting. Next morning, the 23d, no amendment had taken place. Fæces and oil were discharged by the mouth, but no stool had been procured, and hiccup had commenced. Dr Ebers then considering the case almost hopeless, administered an ounce and a half of quicksilver, and after it ten drops of laudanum, with several small doses of a cathartic emulsion. Two ounces more of mercury were given in a few hours. Not long after this the pain abated a little, and the vomiting became less incessant; but at the same time the pulse rose, the countenance became florid, and the abdomen so tender, that friction and fomentations could not be endured. Eight ounces of blood were therefore drawn, which checked the febrile state, but caused great weakness. Neither on this nor on the former occasions was the blood buffy. Clysters now administered brought away a little fecal matter, and a gurgling was heard in the intestines; but cathartics did not cause any discharge from the bowels. At night no amendment was visible. Dr Ebers ordered three ounces more of quicksilver, and then other remedies to be continued, but with hardly any hope of her recovery. At two in the morning of the 24th, however, she fell asleep, and remained quiet for two hours, when at length copious feculent stools were discharged with amazing relief to all her sufferings. The vomiting ceased, the pain became inconsiderable, the pulse fell to the natural standard, a gentle perspiration broke out, and she was able to quench her urgent thirst with the utmost gratification. Fæces were discharged throughout the subsequent day in large quantity. The first stools unfortunately were thrown away; but no quicksilver could be seen in the others. On the 25th the belly was doughy and excessively tender, and the stools scanty. Although she was extremely exhausted, the purgatives were repeated, as well as the clysters, by which great quantities of feculent matter were discharged, yet without lessening the size of the abdomen, or the turgidity of the whole colon. A tendency to vomiting had recurred. Opium and calomel, with croton-oil, were then given, after which she became easier, and discharged more fæces. On the 26th the abdomen was soft, and not painful. On the 27th she continued to improve, and still discharged enormous quantities of fæces, amidst which a few globules of quicksilver were for the first time observed. From this time forward the stools became loose, though copious; afterwards a diarrhœa set in. Tonics were therefore ordered, including wine. In the middle of October, with the exception of some remaining weakness, her health was quite re-established. On the 1st November an ounce and a half of mercury were discovered in the stools, and about as much more was discharged in the four following days. Although the mercury had thus remained five weeks and a half in the body, no inconvenience was produced by it, and in particular no salivation.—*Case Second.* In the summer of 1812 a woman was brought in the afternoon into the hospital of Breslau on account of an obstinate constipation. She had been several days ill with this complaint, combined with dreadful pain in the belly, and incessant vomiting; and on the day of her admission into the hospital she vomited a large quantity of feculent matter. Her countenance was sunk, the skin and extremities cold, the pulse small and quick, her strength excessively exhausted. The belly was everywhere extremely tender and doughy; and Dr Ebers imagined he felt large masses of fæces through the parietes. Her sufferings were so acute that she cried continually aloud, and could be persuaded to be silent for a few seconds only. A warm-bath was tried, but was of no use; clysters were not retained; and every sort of medicine was rejected from the stomach immediately after being swallowed. A little opium was administered, but to no purpose. As the case appeared hopeless, Dr Ebers resolved to have recourse to mercury, although he admits that he could not elicit any indication of probable advantage from it. Four ounces were given without any thing else; and the patient swallowed it very unwillingly. In a few minutes she became a little quieter. In an hour two ounces more of the quicksilver were given, after which she fell asleep. She had scarcely been half an hour in this state when she discharged so enormous a quantity of fetid fæces, that it was necessary to remove her at once to another bed, while many of the patients vomited from disgust, and the physician himself was unable to remain a few minutes beside her. Several other evacuations speedily followed, in all of which mercury was found, though not so much as she swallowed. The patient recovered with great rapidity; she could hardly be persuaded to take a few doses of laxatives; and in eight days she was dismissed.

The rest of Dr Ebers' paper contains much interesting historical and pathological information on the subject of ileus, and the correlative diseases of the bowels. But this part of it we must reluctantly pass over without notice. He has entered at great length into the various explanations that have been proposed to account for the operation of quicksilver in ileus, and ends with confessing that none are satisfactory. He admits that in very many forms of the disease this remedy is useless, and conceives that it is likely to prove advantageous only in the spasmodic species, and in that which depends on fecal obstruction. In these varieties he supposes the remedy acts by percolating the contents of the canal till it accumulates at the obstruction, and then by its weight and distending power presses the parietes, and interrupts the retrograde spasmodic action of the intestinal muscles. On account of the uncertainty of the diagnosis as to the cause of ileus, he allows that the physician must act in the dark, and therefore thinks quicksilver ought not to be resorted to, except as a last resource; but at the same time he insists that it never does any harm. When it is to do good, its beneficial effects begin in a few hours, or even less than an hour. On examining the results of prior authors who notice this mode of treatment, he finds that those who commend it used large quantities, while those who consider it of little advantage commonly gave it in small doses.

LEGAL MEDICINE AND SATISTICS.

Poisoning with the Salts of Morphia. (*Revue Médicale, Septembre* 1829.)—M. Julia Fontenelle relates two instances of poisoning with the preparations of morphia.—One of these, which proved fatal, originated in a mistake committed by a physician, who prescribed ten grains of *sulphate of morphia* in the way of injection for a child five years old. The apothecary to whom the prescription was sent represented to the grandmother of the child that the dose was too large; but she had so much confidence in her physician, that in reply she requested the injection might be made according to the terms of the prescription. Soon after the injection was given, the child fell into an apoplectic state, and, notwithstanding all the resources of art, expired in eleven hours.—The second case is a much more remarkable one, as the particulars have been recorded in detail. A Brazilian student of medicine, urged by domestic distress to put an end to himself, purchased twenty-four grains of acetate of morphia at six in the morning, and swallowed twenty grains of it dissolved in an ounce of distilled water. In six or seven minutes the symptoms of poisoning commenced with loss of consciousness. He remained alone till four o'clock; at which time the master of the lodging-house being surprised at his non-appearance, directed the door to be opened, and found him stretched on his bed, and in dreadful convulsions. Professor Orfila and several other physicians were soon summoned to his assistance. They found him affected with violent trismus, so that not a drop of liquid could be introduced into his mouth, the pupils slightly dilated, the whole body chilly, the pulse 120, the breathing impeded and stertorous, the skin itchy. As it was impossible to administer emetics, venesection was performed to a large amount, (seven *pallettes*,) after which the body became warmer. Sinapisms were then applied to the feet, and injections of tartar emetic administered. The patient in consequence became able to swallow vinegar and water, and infusion of coffee. At ten at night he recognized M. Orfila. The frequency of the pulse, as well as all the other symptoms of poisoning, had abated very much. In four days convalescence was completely established. Orfila stated that he had never seen so violent a case of poisoning with opium or any of its preparations.

Poisoning with Acetate of Copper. (*Revue Médicale. Septembre* 1829.)—A lace-worker, twenty-six years old, of a very melancholic tendency, and who had attempted to destroy himself with the water-hemlock five years before, put eight *sous* pieces into a glass of strong vinegar, and left them there for seven days. At two o'clock of the afternoon, having made a good dinner, he drank first one-half, and then in fifteen minutes the remainder of his potion. Not content with this, he washed the coins in more vinegar, brandy, and anis-seed water, all of which he also swallowed. Three hours afterwards he was fonnd insensible. The muscles were violently convulsed; the limbs in the intervals of the convulsions were stiff, the teeth set, the breathing interrupted, the pulse small, hard, and very slow, the pit of the stomach tender on pressure. With difficulty he was made to drink some glasses of hot water, which, however, did not make him vomit. In half an hour he recovered his senses, and told what he had taken. The white of eggs was immediately given in large quantity. The convulsions afterwards ceased rapidly, but he continued to hiccup till two in the morning. Next morning the pulse was full, slow, and intermitting: the belly drawn in, hard, and very painful on pressure; the skin pale; the convulsions partial and transient. Leeches were directed to be applied to the abdomen, and afterwards poultices; at the same time the white of eggs was persevered in; and the warm bath, with mild opiate injections, was used. In the evening he had colic, dyspnœa, great agitation, hiccup, and a hard contracted pulse. The leeches were repeated. The urine at this time was scanty and scorching. He passed an indifferent night; but next morning was easier. The abdomen was not tender, the pulse was soft, and the urine was natural in quantity. In fourteen days after his admission he was dismissed cured.

Professor Orfila on the Detection, Morbid Appearances, and Treatment of Poisoning with Hydrocyanic Acid. (*Annales d'Hygiène Publique et de Médecine Légale, Juillet* 1829.) If the hydrocyanic acid is in a state of pure solution in water, the most delicate tests for it, according to the late researches of Orfila, are nitrate of silver and the smell. Its peculiar smell is perceptible when no chemical reagent is delicate enough to detect it. The nitrate of silver causes in very diluted solutions a white precipitate, the cyanide of silver, which is insoluble in nitric acid at the temperature of the air, but easily soluble at the temperature of ebullition of the acid, very soluble in ammonia, not easily blackened by light, and yielding, when heated under exposure to the air, metallic silver and cyanogen gas. As the latter is easily recognized by prominent properties, Orfila thinks that the nitrate of silver is a very characteristic test of the presence of hydrocyanic acid. The sulphate of iron, which is usually employed, he finds to be less delicate. This is used by adding an alkali to the liquor supposed to contain the poison, and then dropping in the sulphate; a greenish brown precipitate is formed which becomes Prussian blue on the addition of a little sulphuric acid. It is very strange, that the professor continues to repeat the error into which Lassaigne and other French chemists have fallen, and which was pointed out some time ago by Dr Turner,—that the proper sulphate for this purpose is the *per*-sulphate. The correct test is the *proto*-sulphate or common green vitriol; the true persulphate having no action at all except with the alkali added to the suspected solution. We entertain great doubts of the accuracy of his statement as to the delicacy of the test of the smell, provided the acid be pure and not mixed with etherial oil, as in the oil of almonds or cherry-laurel water.

If the hydrocyanic acid is mixed with a coloured solution of such a tint that the nitrate of silver and sulphate of iron cannot act characteristically, it will sometimes suffice to hold for a few minutes in the fluid a bit of paper impregnated with pure potass, and then to touch it with a solution of sulphate of iron, when the blue colour will be struck on the paper. On other occasions the fluid may be sufficiently decolorized with animal charcoal used without heat. When neither of these modes will answer the liquid must be gently distilled, and the tests applied to the distilled fluid.

If the medical jurist is required to determine the quantity of hydrocyanic acid in a solution, which, on account of its very variable strength, may often be necessary in judicial proceedings, the best method, according to Orfila, provided the solution is pure, or contains only syrup or such other articles as do not precipitate with nitrate of silver, is to add that reagent in excess, and to dry the precipitate. Of this precipitate, which is the cyanide of silver, 100 parts when properly dried are equivalent to 20.33 of pure hydrocyanic acid.

On the subject of the morbid appearances caused by hydrocyanic acid the only new information of material value in the present paper is an account of the inspection of the bodies of seven persons who were killed by this poison accidentally in one of the Parisian hospitals. The quantity swallowed by each was twenty grains of Vauquelin's acid. The posterior part of the body was livid; the head, face, and lips were violet and a little bloated; frothy blood of the usual colour issued from the nose and mouth; the eyes were closed; and the whole body was (twenty-four hours after death) rigid as usual. In the sub-peritonæal cellular tissue of the stomach and small intestines there was much vascular injection, which near the ileo-cœcal valve gave the gut a black colour. The contents of the stomach did not exhale the odour of hydrocyanic acid. Its inner membrane presented red patches on the prominences of the rugæ and a granulated appearance, as if from enlargement of the mucous crypts. The inner membrane of the small intestines was in a similar state; and at the point corresponding with the external blackness already mentioned there was blood effused between the mucous and muscular coats. The liver, spleen, and kidneys were healthy, but unusually gorged with black blood. The heart was also healthy, and did not contain any blood; and the great arteries were also empty, but the great veins gorged with black fluid blood. The lungs were slightly gorged behind with blood; the windpipe and its ramifications of a deep red colour, and the latter filled with a bloody froth. The scalp when cut into discharged a moderate quantity of fluid blood; the sinuses of the dura mater were full of it; and the vessels in the substance of the brain were in a similar state; but nowhere could any effusion or local redness be seen. The brain was soft. The spinal chord was in the same state with the brain. In no part of the body was there to be perceived any odour of hydrocyanic acid. Putridity had not commenced.—The patients lived from thirty to forty-five minutes after taking the poison; its effects began in about ten minutes; and the symptoms were in all at first convulsions and then coma.

Of the reputed antidotes for hydrocyanic acid, Orfila has found, that liquid ammonia introduced into the stomach, oil of turpentine, and infusion of coffee, are of no use whatever. But diluted ammonia when inhaled will sometimes preserve from death, when the dose has not been sufficiently large to act with rapidity. A dog weighing eight pounds, which had taken eight drops of the medicinal acid in twelve drops of water, and which would have been killed by that dose in twelve or fifteen minutes if left without assistance, was saved by the inhalation of ammoniacal water. Another dog, of the weight of thirty pounds, swallowed fourteen drops in two doses, and was seized violently with the usual symptoms. Nevertheless, by means of the ammoniacal solution, which it was made to inhale by sprinkling it on the muzzle, it recovered in the course of sixty-five minutes. In using ammonia for this purpose care must be taken to employ it sufficiently diluted; for strong ammonia will inflame the mouth, air-passages, and even also the mucous membrane of the alimentary canal. The pure *aqua ammoniæ* should be diluted with twelve parts of water. Professor Orfila has omitted to mention, that the effect of the inhalation of ammonia as an antidote for this variety of poisoning was pointed out for the first time, and several years ago, by Mr John Murray of London.

Very lately M. Siméon, apothecary to the hospital of St Louis at Paris, was led to suppose, from some experiments on animals, that the vapour of chlorine might prove a useful antidote. M. Orfila confirms that idea. He has found that the inspiration of water impregnated with a fourth of its volume of chlorine gas is a more certain and effectual antidote than ammonia, and that animals which have taken a dose large enough to cause death in fifteen or eighteen minutes, will be saved by it, although its administration be delayed till four or five minutes after the swallowing of the poison. A dog of the weight of twenty pounds took six drops of the medicinal acid diluted with eight of water, and in one minute was under the action of the poison, being affected with violent convulsions, spasmodic rigidity, and opisthotonos. In one minute more the chlorine water was administered by inhalation, five minutes after which the flaccidity and insensibility, which followed the earlier symptoms, had almost entirely disappeared. In ten minutes more the animal was quite well. Another larger dog, after taking repeated small doses of the poison without being affected, swallowed nine drops diluted with twelve of water. In twenty seconds it was violently attacked with giddiness, panting, and opisthotonos, and it was to all appearance about to perish. It was then made to breathe chlorine water, which was also sprinkled on its nose; and in ten minutes it so far recovered as to be manifestly out of danger. In three quarters of an hour it was quite well.

Professor Orfila has likewise tried the cold affusion recommended by Dr Herbst, as was announced in the last number of this Journal; and he confirms the results obtained by that gentleman, but thinks that it is inferior in efficacy to the inhalation of chlorine. The treatment of a case of poisoning with hydrocyanic acid should consist, according to the preceding researches, on the one hand, in the inhalation of chlorine if it can be procured, or, failing that, the inhalation of ammonia properly diluted, and on the other hand, in the employment of occasional cold affusions. It is hardly necessary to add, that the ammonia will commonly be preferred to the chlorine, as the latter will very rarely be at hand. Venesection is also indicated by the signs of venous congestion.

On the Effects of Tobacco on the Health of the Workmen who Manufacture it. (Annales d' Hygiène Publique, et de Médecine Légale, Avril 1829.)—It has long been a matter of deep regret to the statistical inquirer, the political philosopher and the physician, that so little definite information is possessed on the healthiness and unhealthiness of the different trades; and it has been the subject of great surprise that advantage has not hitherto been taken of the ample means of investigation in the manufacturing parts of the British empire to supply this important defect. Every one must peruse with astonishment even the most modern works on the diseases of artizans, where he is led to infer that there is hardly a trade in civilized life, which is not calculated to induce many peculiar maladies, and materially to shorten the term of existence, while nevertheless when he takes a general view of the state of the population engaged in trade, he is unable, except in a very few instances, to detect any satisfactory evidence in support of such belief. Now that medico-statistical inquiries begin to attract in this country the deep attention which they merit, we may hope to see these inconsistencies and our defective knowledge removed. Meanwhile the subject has been taken up, apparently with zeal and activity in another land, where we are compelled in justice to say that in almost all the departments of medicine, essential and collateral, there at present prevails a spirit of enterprize and an assiduity of investigation, which is far from being so generally diffused at home. In Paris a particular department exists under the name of the *Conseil Général de Salubrité*, which is composed of several of the chief members of the state and many of the eminent physicians, surgeons, chemists, and engineers, and which has a general charge of all matters relative to the public health. The Journal from which the following observations are quoted, which is of very recent origin, and one of the most valuable periodicals at present published in Europe, contains several most interesting narratives of the labours of this body in various departments. Among other topics the General Council has begun to investigate the healthiness of the trades where great numbers of individuals are collected together; and the first produce of their exertions is a long and admirably written paper by *MM. Parent-Duchatelet and D'Arcet*, two of the members, *On the Real Influence which Tobacco has on the health of the workmen who manufacture it into its various commercial forms.*

in regard to many other trades, so with respect to that of tobacco-manufactu-
rong prejudices have been long entertained against it on account of its sup-
unhealthiness. The authors quote various prior writers, from Rammazzini
to Patissier, who all support the vulgar opinion. But their statements are
in the highest degree, their facts scanty and imperfect to a lamentable ex-
nd their whole inquiries as rich specimens as can well be imagined of hasty
ion and meagre thought. Among the authors quoted, we find the names of
roy, Cadet-Gassicourt, Tourtel, Percy, and Mérat.

I. Parent-Duchatelet and D'Arcet set out in the narrative of their inquiries
ecifying the several processes to which tobacco is subjected in the course of
acture. 1. The first process is the shaking out the bales, during which
tobacco dust is raised. 2. The next is the damping of the leaves. 3. The
the removal of the footstalks and greater nerves of the leaves, which is done
knife or razor. 4. They are next gathered in heaps from thirteen to nineteen
uare and high, which are moistened with weak brine and other articles to com-
ate fragrance and then allowed to undergo slight fermentation. At this stage
process a different procedure is observed for the manufacture of snuff, and
t of tobacco for chewing and smoking. 5. The large leaves are picked out
ars and chew-twists, while the remainder is cut into fragments for pipe-smok-
The tobacco for these purposes is dried by smoke on iron tables. 6. For mak-
uff, the heaps are coarsely chopped down with knives and hatchets, next put
e hands of the raspers and grinders, and then passed through the sieve. 7. Ad-
l qualities are communicated to the powder in large wooden boxes, by various
ulations.—In the first of these processes the workmen are constantly exposed
cco-dust; in the second, they work in low, damp apartments; in the third,
t on and are surrounded by great heaps of tobacco; in the fourth, great cor-
force is required to form the heaps; in the sixth, the workmen are often wound-
their cutting instruments, and are exposed to an atmosphere of hot, acrid
rs, disengaged by the breaking up of the fermenting heaps. The powdering
tobacco for snuff, when not accomplished by mechanical means, requires vio-
ertion, so as to cause continued sweating. The sifters, and those who carry
ted powder to the boxes, are exposed to strong acrid vapours, which no un-
ed person can endure. Those who superintend the drying of the tobacco for
ng, are exposed to a great heat, and the most pungent vapours disengaged in
ole manufactory.

order to ascertain the effects of these operations on the workmen,—opera-
which it is natural to suppose *à priori* would be injurious, the authors
d for authentic returns from all the great snuff manufactories in France.
are ten in number, and are situated in Paris, Toulouse, Lyons, Strasburg,
illes, Touneins, Bordeaux, Morlaix, Lille, and Havre. The workmen em-
are 4518; so that the field of observation was sufficiently extensive. The
oint relative to their health which engages the attention of MM. Parent-Du-
et and D'Arcet is the answers given by the managers and physicians of these
ishments to the question, *whether the workmen experience any change in their*
f health when they commence the trade of tobacco-manufacturer and have not
abituated to its emanations? At Paris, where the authors had an opportunity
sonally questioning the workmen, it appears that very few are incommoded,—
ome of the workmen who cut down the tobacco-heaps and dry it have been at-
with vomiting, stomach complaints, and diarrhœa,—that they almost inva-
get rid of these inconveniencies in a few days,—and that it is extremely rare
workman to be compelled to abandon the trade. In most of the manufacto-
such instance had occurred in a long series of years; in others one or two
Even children are not affected.—The next question which is examined in
esent paper is, *whether the health of the workmen is injured by their trade?*
s question the invariable answer from every establishment is, that the workmen
t subject to any disease to which workmen generally are not subject, and that
re not more subject than others to certain diseases, or to disease generally. They
or the most part a healthy look, and if they are seldom corpulent, this arises
m any attenuating property in tobacco emanations, but from the hard work to
they are subjected. These practical results are strangely at variance with the
tical statements which have been hitherto received as true,—that the workmen
bject to colic, rheumatism, hemorrhagies, nausea, giddiness, muscular tremors,
ffections, and asthma at an early age. From an account kept at Paris of

the state of sickness of the work people, it follows that the average number of days when each was off work on account of sickness was only $11\frac{3}{8}$; and in five years out of a population of 1050, the annual mortality has only been 2.6, or *one in* 404.—The third question is, *what effect has the tobacco on the functions of the nervous function?* It has been supposed by many that the narcotic properties of tobacco ought to exhibit themselves in the form of stupor and somnolency among the workmen. But on the contrary, it is expressly stated that they are often found sleeping on the tobacco-heaps, and that neither asphyxia nor sopor was ever observed to arise from the practice. At some of the establishments the manager and his assistants as well as the physician have made the experiment with the same result.—The next question relates to *the causes of the invaliding of workmen from the manufactories.* These are old age, insufficient strength, palsy, epilepsy, scrofula, dartrous eruptions, maims; but very few have been dismissed for any of these causes except old age; and at Paris very few claims to retire on the superannuated fund have been made before the age of sixty.—To the next question, *whether the trade impairs longevity?* it is replied, that many old men are on the manufactory lists, very many above seventy, and that a great number of these had been 30, 35, and 40 years in the trade. At Havre, in a population of 348, there are 107 above fifty years of age. At Paris, in a population of 1050, there are 152 above sixty, 101 above sixty-five, 44 above seventy; and of the 152 only 36 had worked for less than fourteen years in the manufactory.—The last question, is, *whether in respect of the public health, tobacco manufactories ought to be tolerated in great towns?* To this question it is answered, that on one condition they are perfectly harmless; and the condition is, that the refuse of the tobacco-bales be burnt at a distance from the town. This operation appears to be one of the greatest nuisances which can exist in the neighbourhood of any human habitation. It is stated in the present paper, that the acrid vapour disengaged by the furnaces for consuming the refuse of the Paris manufactory, renders it intolerable to reside even within a mile of the place where this operation is conducted. This fact will surprise no one when it is mentioned that the matter burnt usually amounts each time to 500,000 pounds, and that the burning continues eight days and upwards. The burning is performed in the Plain of Grenelle; and repeated and well-founded complaints have been lodged with the authorities by the surrounding villages. But around the manufactories themselves no complaints of nuisance have ever been made, when this operation was not conducted in them.

Statistical Account of the Lunatic Asylum at Charenton for the years 1826–7–8. [*Annales d'Hygiène Publique et de Médecine Légale, Avril* 1829.]—M. Esquirol, who has been for some time chief physician to this establishment, has published a long report on the state of the patients treated there in the years 1826–7–8. The following is an abstract of the chief numerical statements. The Asylum at Charenton, (Maison Royale de Charenton,) was founded in 1641 by Sebastian Leblanc. At first a general hospital, it afterwards became an asylum; and in 1790 contained between 80 and 90 patients. At present, its population fluctuates between 480 and 500. Many improvements have been made of late years in its various establishments, and more are projected; so that, considering also the beauty of the situation, it will soon become one of the most perfect asylums for the insane in Europe. The patients received into it are chiefly from the better ranks of life.

The admissions during the three years specified above have been 619, or 206 annually. The greatest number of admissions have been in the three hot months of June, July, and August, namely 31.5 per cent of the admissions per year; next to these months are March, April, and May, when the admissions were 25 per cent; next to these, December, January, and February, when 22.3 per cent. were admitted; and the lowest were September, October, and November, when only 21.2 per cent. were admitted.—As to the ages, 3.56 per cent. were between fifteen and twenty; 10.83 between that and twenty-five; 13.9 between that and thirty; 15.84 between that and thirty-five; 13.09 between that and forty; 12.76 between that and forty-five; 11.63 between that and fifty; 8.4 between that and fifty-five; 3.4 between that and sixty; the same between that and sixty-five; 0.96 between that and seventy; and 2.23 per cent. above seventy.—With regard to the sexes, it is a remarkable fact, that (contrary to what is observed throughout France generally, where the women preponderate in the immense disproportion of 14 to 11 males,) at Charenton the males preponderate over the females in the ratio nearly of 3 to 2. On the subject of the influence of sex, M. Esquirol compares the differences in different countries, (which the reader will remember were brought before the public by Dr Burrows;) and he arrives at the singular conclusion, that the differences compensate one another, so that taking in one mass the insane population of France, Spain, Italy, Holland, Britain, the north of Germany, and the United States of America, out of 76,526 insane, 37,825 are males, and 38,701 are females, which is nearly in proportion to the relative number of the sexes in the population generally. A singular fact deduced from the lists at Charenton, is, that after the age of sixty-five few women become insane, compared with the number of men. Of fourteen patients admitted after that age, three only were females. All of these patients were maniacs or monomaniacs.—Nearly half of the 619 were unmarried. Of 282 married persons, exactly one-half were males, and one-half females: But of 293 unmarried persons, there were 206 males and only 87 females. This extraordinary difference shows how much the excesses of a single life add to the list of the insane among the middle ranks.—As to profession it appears that a great number of the insane males belong to those trades in which the body is much exposed to heat, such as that of baker, innkeeper, and cook. Among the women those are most liable who lead a sedentary life, working in their own houses. A singular fact is, that among the military as many insane are supplied by the officers as by the privates. The greater number of the insane military throughout France are transmitted to Charenton; and during three years there were admitted 49 officers and 47 privates.—The Physical Causes could be traced only in 256 cases, among which 93 were traced to hereditary tendency, 23 to masturbation, 24 to libertinage, 16 to the abuse of mercury, and 64 to drunkenness. The operation of moral causes could be traced in 192 only; and of these 89 arose from domestic distress, 20 from reverse of fortune, 21 from crosses in love, 18 from exalted devotion, 13 from jealousy, 8 from excessive study and watching, 7 from fright, 7 from novel-reading, 6 from crossed vanity, 2 from passion for gambling.

Of the four forms of insanity, monomania, mania, demency, and idiocy, the proportion per cent. was 46.7, 36.5, 16, and 0.6. Mania and demency were proportionally most frequent among males, and monomania among females. Of 366 males, 40 per cent. were maniacs, 38 per cent. monomaniacs, and 22 demented; while of 258 females, 31 per cent. were maniacs, 58 per cent. monomaniacs, and $7\frac{1}{3}$ demented. The admissions on account of monomania and demency are pretty equally dispersed throughout the hot and cold months. But mania is much more common in the former. Of 226 cases of mania, 64 were admitted in the four first months, and 62 in the last four; but in May, June, July, and August, 101 maniacs were admitted.

The cases discharged were 624. Of these 209 were cured, 194 were restored to their friends, and 221 died.

The cures were therefore one-third of the total admissions,—a proportion which is very favourable to the establishment, when it is considered that cases of every denomination are received. If from the number of admissions those cases be subtracted which from the beginning were incurable, (that term for the sake of certainty being applied only to paralytics who were 119 in number, epileptics who were 19, and idiots who were 4,) then the cures among the remainder were two-fifths. It is remarkable that of the insane military one-half were cured.—The cures were proportionally as numerous among the men as among the women; which is contrary to the general fact, the females cured being generally found more numerous in proportion to the total number than the males.—Of the 209 cures, 115 or more than a half were maniacs, 91 were monomaniacs, and three cases of demency. Better information on the relative curability of the several forms is derived from comparing the cures with the admissions of each quality. When this is done we find that $\frac{115}{226}$ maniacs were cured, $\frac{91}{289}$ monomaniacs, and $\frac{3}{99}$ cases of demency,—that is 51.66 per cent. of the maniacs, 31.8 monomaniacs, and 3.0 demented. Nearly the same proportion of female maniacs are cured as of male maniacs. M. Esquirol, indeed, states that more male maniacs are cured than female maniacs, and more female monomaniacs than male monomaniacs. But his statement is calculated to convey a false idea of the fact. The actual *numbers* stand as he represents; but the *proportion* of maniacs or of monomaniacs cured in each sex gives a very different result. In fact, the same proportion of the male manomaniacs is cured as of the females; and the same proportion of the female maniacs is cured as of the males.

The deaths were 221. In judging from this fact of the mortality at Charenton, a comparison must not be instituted with the admissions, because a considerable number of the deaths took place among 492 patients remaining in the Asylum on the 1st January 1826. The only just idea of the mortality is obtained by comparing the number of deaths annually with the population. It will then be seen that on an average 73⅔ die annually in a population of 490, that is 15 per cent. The mortality is proportionally greater among the men than among the women. For while the admissions are 60 males to 40 females, the mortality is 70 males to 30 females. This difference appears to depend on the greater frequency of paralysis among the males, which is invariably a fatal complication. Of the deaths 52 per cent. were cases of demency, 27.1 per cent. mania, and 19.5 monomania. There were only three cases of suicide in the three years.

The experience of M. Esquirol as to the morbid appearances found after death is on the whole strongly confirmatory of his former statements on the same subject. "The lesions of the encephalon, says he, bore no relation either to the character of the delirium, or the violence of the symptoms. Insane persons whose delirium and infirmities might have led one to anticipate numerous and extensive organic derangements of the brain, have sometimes presented only slight deviations from the healthy structure; while others scarcely delirious at all, and without any bad symptoms, have presented organic lesions of great extent and variety. It is peculiarly disconcerting to all theories on the seat of insanity, even to those which rest on the most numerous and best observed facts, that often enough no appreciable departure from the healthy structure can be detected in the brain and its membranes; and that this absence of organic derangement is sometimes met with in those who have passed through all the stages of delirium, and have lived in a state of insanity several years." Of 199 inspections M. Esquirol found disease of the brain or its membranes in 163, diseases of the lungs, heart, or their membranes in 116, and disease of the abdominal viscera in 113 cases.—In two cases the patients died of suffocation from food or other articles sticking in the pharynx or gullet. This has been at Charenton by no means an uncommon accident; and the author conceives that many of the instances of supposed sudden death from syncope during meals are cases of the same description. Sometimes it depends on organic disease in the pharynx or gullet, but much more commonly on paralysis.—General paralysis has been a very common complication in the cases at Charenton. Of the 619 admissions 109 had this affection. Of these ninety-five were men. The proportion is much greater therefore among males than among females. The proportion, he has also found, is greater among the males of Charenton, than among those at the Bicètre in Paris, where the poorer orders only are received. These facts appear to depend on the description of persons in whom the complication is most frequently observed, who are those addicted to venereal excesses, or to drunkenness, or who have used mercury immoderately.

On the Adulteration of Milk at Paris. [*Annales d'Hygiène Publique et de Médecine-Légale. Juillet* 1829.]—The subject of the adulteration of milk has been lately investigated with great care by M. Barruel of Paris. Although his observations are intended to apply only to the milk of that city, yet there is little doubt that they will also be found applicable in a greater or less degree in all great towns.

He sets out with stating that all instruments for ascertaining the purity of milk, which are calculated to attain this end by pointing out differences in its density or specific gravity, are inaccurate and useless. For, on the one hand, pure milk differs much in density according to the fodder used by the dairy-man for his cows, the butyraceous matter which imparts lowness of density, being made to preponderate by some sorts of food, and the caseous part, which increases the density, being made preponderant by other sorts. And, on the other hand, although water, the ordinary substance with which milk is adulterated by the dealers in the French capital, would alone cause a great diminution of density, the dealers know very well how to prevent that effect, and so render the areometer useless. For this purpose it is only necessary to dissolve in the milk a little sugar-candy, which is required at all events in order to correct the flat taste imparted to milk by diluting it with water.

The result of M. Barruel's inquiries on the adulteration of milk in Paris is that no positively noxious substance is ever to be found in it, that a common practice is to remove a considerable portion of the cream by allowing the milk to stand for a limited time, and then to dilute the remainder, or skimmed milk, with water, and to give it the apparent qualities of new milk in one or other of the manners now to be mentioned. The opacity of the milk being much diminished by the water, so that the milk acquired a bluish appearance, it was at one time usual to correct this defect by previously mixing wheat flour with the water with which the milk was diluted. But this adulteration was too obvious to the senses. Any person even of indifferent delicacy of palate could detect the altered taste of the milk; and besides, after two hours rest the flour sank to the bottom, restoring the translucent blueness of the milk and pointing out the nature of the fraud. To prevent this inconvenience the dealers boiled the flour in the water before mixing it with the milk; and in this way an opaque mixture was procured which retained its opacity on standing. As even with this addition the fabricated liquid had a flat taste, sugar or sugar-candy was dissolved in it, by which means the peculiar sweetness of the milk was partly restored. This adulteration, however, has become so easy of detection by means of iodine, which renders the mixture blue by its action on the fecula of the flour, that M. Barruel believes the fraud now described is very little practised in the present day at Paris. In Britain, where the municipalities take no charge whatever of the purity of this most important article of food, it may be presumed that the adulteration with flour, sugar, and water is common enough, as it is a simple and cheap mode of accomplishing every purpose of the fraudulent dealer. The best mode of proving the presence of farinaceous matter in such mixtures is to heat the milk with a little sulphuric acid, to coagulate the casein, to filter the whey, and then to add to the latter the tincture of iodine; upon which a fine blue colour will be struck.

Driven from this species of adulteration, the Parisian dealers have latterly resorted to another so ingenious, that M. Barruel conceives they could not have discovered it without the aid of some scientific person. The method is simple, so cheap, that for tenpence the opacity and colour of milk may be imparted to thirty English pints of water, and so far secret that no disagreeable taste is communicated. This is nothing more than the employment of an emulsion of almonds, for which some dealers, more greedy and less cautious than the rest, have substituted hemp-seed, which, however, is apt to impart an acrid taste. By either of these means the milk may be diluted to an indefinite extent; and the only corrective required is a little sugar-candy to remove the flat taste. A peculiar advantage possessed by this mode of adulteration over every other, is, that the vegeto-animal matter, or vegetable albumen of the emulsion by which the oil of the almond is held in suspension, is coagulated, or curdled, like casein by acids. The method recommended by M. Barruél for detecting the fraud is founded on two circumstances,—the greatly inferior quantity of coagulum formed by acids in the mixture of milk and almond emulsion, compared with that formed in milk alone,—and the facility with which, by kneading the coagulum with the fingers, oil may be squeezed out of the former, while none exists in the latter.

On examining carefully four different specimens of pure milk, procured from different quarters of Paris, he found that 300 parts of each, coagulated by heating them with an equal volume of vinegar, gave each a quantity of curd, which, when well-drained, and equably pressed between folds of bibulous paper, weighed 29 parts; and that the same quantity of milk taken from a cow in presence of a person sent to procure it, gave 30 parts of curd. He then found that when the same milk was mixed with various proportions of water, the quantity of curd was exactly in the inverse ratio of the proportion of water added. The water, therefore, did not prevent any portion of the curd from being thrown down by the usual modes of curdling the milk. He next found, that, if a given quantity of sugar was added to the mixture of milk and water, the quantity added could be separated exactly by evaporating the whey to the consistence of an extract, treating this with alcohol, filtering the alcoholic solution, and evaporating to dryness. He then also found that, when equal parts of almond emulsion and milk were mixed together, 300 parts of the mixture curdled by vinegar as above, gave 16½ parts of curd; and that the same quantity of a mixture containing two parts of emulsion to one of milk, gave only 10 4-5th parts of curd. So that although, as was to be expected, the adulteration with almond emulsion did not lessen the quantity of curd to the same extent as adulteration with water only, yet the decrease was very great, and very nearly in the ratio of the proportion of emulsion added. Lastly, on placing pure curd on white paper, no oily matter was thrown out; but the curd procured from the mixture of milk and almond emulsion, besides being less firm than the former, gave out in 24 or 48 hours a quantity of oil sufficient to stain the paper.

Another adulteration to which milk is subjected in Paris is with carbonate of potass or soda. The object of this variety of adulteration is in the hot summer months to prevent the milk from becoming sour and curdling, or to break down the curd and correct acescency when the milk has actually become spoiled. In this process acetate of potass or soda is formed. Neither of these salts in moderate quantity is injurious to the health; indeed, acetate of potass exists naturally in milk, and is the source of some embarrassment in the detection of the present fraud. The mode of analysis adopted by M. Barruel is as follows. As the alkaline acetates are converted by incineration into carbonates, he endeavoured, by means of this property, to ascertain the quantity of alkali naturally contained in whey. He therefore evaporated a certain quantity of whey to dryness, incinerated the residue in a platinum crucible, and procured an alkaline ley from the remainder, which, by the process recommended by Décroisil for measuring the strength of alkaline fluids, indicated from one and a half to two degrees of alkalinity. Hence any increase of alkaline strength above the last of these points must be considered as owing to the intentional addition of carbonate of potass or soda. This is evidently the most difficult of the processes recommended for detecting the various adulterations specified in M. Barruel's paper. Indeed a chemist alone could conduct it. The others may be easily executed by any body.

Dr Ballingall, Professor of Military Surgery in this University, has in the press Five Introductory Lectures, comprising observations on the means of preserving the Health of Soldiers and Seamen—the Examination of Recruits—the Economy of Military Hospitals—the means of transporting sick and wounded, &c. &c.

Mr Charles Bell has in the press, in one vol. 4to, a work on The Nervous System, containing his Papers read before the Royal Society; with Engravings, and an Appendix of Cases and Consultations, illustrating the Doctrines advanced in the Text.

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Communications have been received from Drs FLEMYNGE PORTER, BOSTOCK, SHEARMAN, Mr RHIND and Mr HOPE.—We regret that the posthumous communication of Dr Goodwyn came too late for insertion in the present number; but we beg to assure Dr Bostock that it will appear in the next.—To Dr Briggs we beg to say, that we have inspected several of the hospital journals for the period alluded to, but without finding the case to which his communication refers. We are authorized, however, to say, that Mr Russell, our indefatigable professor of Clinical Surgery, whom we consulted on the occasion, remembers the case distinctly, but cannot speak positively as to the interval between dislocation and reduction. It is our intention, therefore, to inspect the books once more before we admit that no record of the case is preserved.

The following Publications have been received :—

Practical Remarks on Amputations, Fractures, and Strictures of the Urethra. By Stephen Love Hammick, Surgeon Extraordinary to the King, and late First Surgeon of the Royal Naval Hospital at Plymouth. London, 1830. 8vo.

Introductory Lectures to a Course of Military Surgery, delivered in the University of Edinburgh. By George Ballingall, M. D., F. R. S. E., Regius Professor of Military Surgery, &c. &c. Edinburgh and London, 1830. 8vo.

The Art of Prolonging Life, in which the subject is fully considered, both Philosophically and Practically. By Christopher Hufeland, M. D. A new Edition, with Notes. By An English Physician. London, 1829. 12mo.

A Popular Summary of Vaccination, with reference to its efficacy and probable causes of failure; as suggested by extensive practical experience. By John Marshall, Esq. Member of the R. C. Surg. London, &c. London, 1830. 8vo.

Observations on the Functional Disorders of the Kidneys, which give rise to the formation of Urinary Calculi; with remarks on their frequency in the county of Norfolk. By William England, M. D. &c. &c. London, no date. 8vo. Pp. 108.

A Dissertation on the Influence of Heat and Humidity; with Practical Observations on the Inhalation of Iodine, and various vapours in Consumption, Catarrh, Croup, Asthma, and other Diseases. By James Murray, M. D. Member of the R. C. S. &c. London, 1829. 8vo. Pp. 305.

Illustrations of some of the principal Diseases of the Ovaria, their Symptoms and Treatment; to which are prefixed, Observations on the Structure and Functions of these Parts in the Human Being, and in Animals. By Edward J. Seymour, M. D. Fellow of the Royal College of Physicians of London, and one of the Physicians to St George's Hospital. With 16 Lithographic Engravings. Lond, 1830. 8vo. Pp. 126.

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THE

EDINBURGH

MEDICAL AND SURGICAL JOURNAL.

1. APRIL 1830.

PART I.

ORIGINAL COMMUNICATIONS.

Art. I.—*Quarterly Report of the Edinburgh Surgical Hospital from November* 1829 *to February* 1830. By James Syme, Esq. Fellow of the Royal College of Surgeons London and Edinburgh, and Lecturer on Surgery in Edinburgh.

Within the last three months, viz. from the 8th of November to the 8th of February, 410 patients affected with surgical diseases have applied to the Hospital for relief. Of these 67 have been admitted into the house, and the remainder have been treated as out-patients. In next Report I will give a general statement for the whole year of the diseases which have been presented at the Institution. In the meantime I will notice what were most remarkable in a professional point of view; and having in last Report confined myself to the subject of ulcers, I will now mention some of the more interesting cases that occurred during the period to which it referred, along with those of the quarter at present more particularly under consideration.

Fracture of the Thigh.—Of this accident there have been five cases, three of which were very similar to each other, the subjects being men in the vigour of life; the fractures being at the lower third of the thigh; there being much tendency to retraction; and lastly, all of them being attended with a considerable dropsical effusion into the knee-joint.

The treatment consisted in the application of pasteboard splints, padded with tow, on each side of the limb, to prevent displacement from its weight and the spasmodic action of its muscles, and a long splint of wood extending from the false ribs beyond the foot, to effect extension.

The cure was complete in all of the cases, though somewhat delayed in one of them, owing to an accident which happened to the patient about a month after he entered the Hospital, when the bone appeared quite firm, and was so straight that my pupils could not discover any difference between it and its fellow. He unfortunately attempted to walk while no one able to afford him assistance was present. He grew sick, fainted, and in his fall fractured the bone a second time. The treatment was renewed, and at the end of two months from the date of admission he was dismissed perfectly cured. The fourth case of fractured femur occurred in Peter Yule, æt. 35, and was complicated with a stiff knee-joint, which prevented the limb from being straightened and extended in the usual way. The fracture, a few inches above the knee, was fortunately transverse, so as to admit of no retraction, and a cure not less satisfactory than that of the others was obtained through means of pasteboard splints, while the limb was laid in a bent position on its side.

The fifth case of fractured femur was that of Jasper Stevenson, æt. 72, farm-servant of Sir Alexander Maitland Gibson, who, owing to a fall from the top of a hay-stack, sustained a fracture of the neck of the bone. The limb was extended by means of the long splint, and matters seemed to be proceeding favourably for about a week, when symptoms of gangrene were observed on the sacrum and instep, which of course rendered it necessary to remove all restraint.

The patient regained considerable strength of the limb.

In old people who have suffered this accident there is always danger of inducing mortification from employing the means most effectual for preserving the limb in proper position, viz. the long splint; but this does not seem to me a sufficient reason, as Sir A. Cooper and some other surgeons think, for abstaining from all attempts of the kind, and for leaving the affair entirely to nature. The most prudent practice seems to be, to give the patient a fair chance of recovery, by subjecting his limb to the proper apparatus, and if he should prove unable to bear it, then there can be no occasion for regret, either on his part or that of the practitioner. Last winter two of my apprentices, Messrs Wishart and Brown, succeeded in curing an old woman of 63, who had suffered a well-marked injury of the kind in question. She can walk well without assistance, and there is no perceptible difference between the two limbs, except a very slight eversion of the toes.

Fracture of the Humerus.—Of this accident there were five cases, viz. one at the neck, three through the shaft, and one just above the condyles. The first occurred in an old woman who was knocked down, owing to her own inattention, by a gentleman riding quietly along the road. She obtained a speedy and perfect cure, without the slightest displacement, in somewhat less than three weeks, by means of a spica bandage, a cushion in the axilla, and a sling. The second case happened to a respectable tradesman, who fell from the height of twenty feet. He was treated out of bed with pasteboard splints, and in three weeks obtained a perfect cure. The third case was that of an old woman, owing to a fall in the street. It was treated in the same manner with equal success. The fourth case happened to a girl from falling on the ice, and was treated like the others. The fifth case occurred also in a litle girl who was hurt by a log of wood. The fore-arm seemed to be shortened, and the olecranon was unusually prominent, so that at first sight the accident seemed to be dislocation backwards. A very slight extension, however, sufficed to ascertain the truth by removing the deformity so long as it was continued, and permitting it to return so soon as it was relaxed. A bandage with proper compresses retained the bones *in situ*, and the patient was dismissed cured in the course of the third week.

Fracture of the Clavicle.—There were three cases of this fracture, which recovered readily under the method of treatment described in my first report.

Fracture of the Tibia.—Of this usually reputed rare accident there were no fewer than five cases. Of these four happened from false steps, and sudden twists of the leg. The bone gave way about the lower third. There was little displacement, and little difficulty in the treatment, which consisted in applying pasteboard splints, and maintaining the limb in a bent position, which was laid on its outer side. The fifth case was caused by direct violence sustained from falling down a stair which gave way in a Leith brewery. The fracture was situated just below the tuberosity of the tibia, and hence it was necessary to place the limb in an extended position, to prevent the displacing effect of the extensors of the knee-joint, which is so troublesome in this particular case, when the knee is bent.

Fracture of the Fibula.—Of this fracture there were three cases, two in males, and one in a female. The treatment in all of them was the ingenious, simple, and effectual plan of Dupuytren, viz. the application of a long narrow wooden splint on the inner side of the leg, extending beyond the ankle and knee, between which and the tibia a thick compress being interposed, while the knee and foot are drawn to the splint by means of a simple bandage, an effectual resistance is afforded to the displacing tendency of the weight of the limb and peroneal muscles.

Fracture of the Radius.—There was one of several cases of fractured arm which occurred that deserves mention, from having been overlooked for three weeks after the accident. The patient was Mrs Cassels, who came from Kilmarnock to obtain relief for an injury of the wrist, occasioned three weeks previously by a fall on the hand. There was no deformity of the limb; but a fracture of the radius very close to its carpal extremity was readily recognized by the crepitus. Splints were applied, and the patient had the satisfaction of sleeping soundly during the following night, which she declared the pain had prevented her from doing ever since the accident. What makes this case the more remarkable is the circumstance, that, after travelling sixty miles to obtain assistance, she failed in finding it at the institution to which she first applied, where the case was still treated as a strain.

Compound Fracture of the Leg.—The only case of compound fracture was that of George Mackay, porter, a most unpromising subject for recovery from such an accident. His appearance indicated a naturally full habit, with a broken down constitution; his pulse was irregular and intermitting; and he looked as if labouring under chronic disease of the chest. Notwithstanding these unfavourable circumstances, he did extremely well, and five weeks after the accident, the bones were firmly united and perfectly straight.

Fractures of the Ribs, Metacarpus, &c.—Of these there were various cases, but none deserving particular attention, excepting one where there was considerable emphysema, which yielded to bleeding, tartar emetic, and bandaging.

Dislocation of the Humerus.—Of this accident there were four cases, all in stout men, and in all the reduction was effected by manual extension, without the assistance of pullies, which I regard on most occasions as equally unnecessary and inconvenient.

Dislocation of the Thumb.—There was one instance of this uncommon and troublesome dislocation, in the case of a woman who fell on the floor while playing with a child. The first phalanx of the thumb was dislocated backwards on the metacarpal bone. I fastened a silk handkerchief to the thumb by means of the clove-hitch, and then making one person extend, while another performed counter-extension by holding the hand, I pressed with all the force of both my own thumbs on the extremity of the dislocated phalanx. After one or two attempts I succeeded in effecting the reduction.

In the only other case of this accident I ever happened to see, the surgeon found it necessary to divide one of the lateral ligaments of the joint.

Dislocation of the Jaw.—There was one case of this dislocation in a woman who had previously suffered the same accident. Mr Smith reduced it in the usual way.

Excision of the Elbow-Joint.—Little more than a year ago, in November 1828, I cut out a carious elbow, for the first time the operation was ever performed in Great Britain. Since then I have operated in six cases. The profession, I am happy to say, seem to be satisfied with the weight of this evidence, and at least two operations of the kind have been lately performed by different practitioners in Edinburgh. Five of my cases have already been put upon record through the medium of this Journal, and I will now relate the other two.

Elizabeth Johnston, æt. 15, from Falkirk, entered the Hospital on the 26th of August, on account of a disease of the right elbow-joint, which had existed for six months, commenced spontaneously, and increased progressively, notwithstanding the efforts of her medical attendants. It now presented a most formidable appearance, the joint being so much swelled as to measure thirteen inches in circumference, and the arm above being reduced to little more than skin and bone, which made the enlargement seem even greater than it really was. The skin over the olecranon was extensively ulcerated, and at different places, both on the front and back parts of the joint, the probe could be passed into sinuses which extended to the bones. The limb was straight, and nearly immoveable. The discharge was profuse, the pain unceasing, and the irritation so great that the patient's health seemed rapidly sinking. It was plainly necessary to do something effectual for her relief, and both Dr Ballingall and I, though entertaining the most favourable opinion of excision, from what we had seen of its good effects, resolved that any operation short of amputation would be inexpedient in this case, where there was such extensive disease not only of the bones, but also of the soft parts. Being, however, very averse in general to amputating the arm for caries, and feeling particular reluctance to mutilate this unfortunate girl, who was distinguished by the most amiable disposition and interesting appearance, I delayed the operation. In the course of ten days, whether it was owing to a real improvement proceeding from the free vent which had been afforded to the matter by incisions, or was merely the effect of familiarity with the appearance of the joint, I fancied that it was not so hopeless as at first believed, and resolved to make an attempt at excision.

The operation was performed in the manner formerly described, and was attended with very little difficulty, owing to the separation of the surrounding soft parts from the articulating bones, which had been caused by collections of matter. The olecranon was greatly expanded, and, if I may use the expression, completely rotten, so that it crumbled into fragments, which were extracted piecemeal. The radius adhered to the humerus, and was extracted along with it. Before dressing the wound, I observed that the ulnar nerve was partially divided by an oblique incision, and therefore cut it completely across, to avoid the danger of such a wound; and its extremities being then placed in contact, the integuments were stitched together. The patient did extremely well; the wound healed most kindly; the swelling of the joint subsided; she gradually regained its use; and is now, I am happy to understand, restored to perfect health.* For some time after the operation, she complained of coldness and numbness in the ulnar side of the hand, but in process of time got rid of these unpleasant symptoms, probably in consequence of re-union between the extremities of the nerve.

James Page, æt. 8, was recommended to the Surgical Hospital by Mr Ferguson of Auchtermuchty, as a proper subject for excision of the elbow-joint, and was admitted on 2d January. The right elbow was much enlarged, discoloured, and stiff. There were two sinuses opening on each side of the triceps, through which a probe could be passed to the bone. The operation was performed on January 12, in the ordinary manner. The wound healed kindly, and the patient is nearly ready to leave the Hospital.

James Alexander, æt. 9, from Arbroath, entered the Hospital on the 2d February, on account of a disease of the elbow-joint, under which he had laboured eighteen months. The bone can be felt extensively diseased, and the case seems in all respects a favourable one for excision, which will be performed so soon as the parents are informed and give their consent.

James Dennet, came from Dundee to place himself under my care on account of pain, swelling, and redness over the olecranon, which had resulted from a blow on the elbow, received several months previously. On coming to town, he was persuaded to apply to another practitioner, who made a long incision through the skin. When he at length applied to me, a small part of the wound remained open over the olecranon, where there was some swelling and much tenderness on pressure. The patient declared that he was not any better than when he left home. Finding that a probe could be passed to the bone and a little way into its substance, I concluded that a superficial caries of the olecranon was the cause of his distress, and, therefore, after exposing the bone, removed the softened portion with a gouge, on the 9th January. The wound is now nearly healed, and the patient makes no complaint.

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* I was informed to-day that her father having lately died, leaving a widow and six children in very destitute circumstances, she is able to contribute towards their support by tambouring muslin.

Excision of the Knee-joint.—The knee-joint, so far as regards its structure, is an equally favourable subject for excision with the elbow, since there is only one articulation concerned in the disease or affected by the operation, and not a number, as is the case in the wrist or ankle. But the advantages from the operation in this situation are much more questionable than in the shoulder or elbow, since not only is there much less difference between the utility of a natural leg and a wooden one, than between that of a real and artificial arm, but doubts may even be entertained as to the probability of deriving any assistance in progressive motion from the limb, which is preserved by cutting out the knee-joint. With the exception of the two cases operated upon by Mr Park of Liverpool, nearly fifty years ago, and the two cases lately published by Mr Crampton of Dublin, I am not acquainted with any recorded facts to guide us in deciding this question. Each of these gentlemen lost one of their patients, but the others survived and retained limbs so useful, that the owners would not readily have exchanged them for artificial ones. Mr Park's patient, a sailor, was able to ascend the rigging of his ship with the agility peculiar to that profession; and the woman on whom Mr Crampton operated could walk the distance of eight or nine miles without suffering fatigue or inconvenience.

The advantages attending excision of the knee-joint over amputation in the thigh, in addition to the satisfaction of saving a limb, and promoting the credit of surgery, seem to me, *First*, The negative one of saving the patient from the inconvenience of resting his weight upon the *face* of a stump: *Secondly*, The positive one of preserving for him the tarsus, metatarsus, and toes, which constitute an apparatus much more efficient in protecting against the effects of concussion than any artificial one that can be constructed. Influenced by these considerations, I resolved to try the operation in some of those cases of diseased knee which so frequently result from white swelling in young subjects, and are condemned without any ceremony to amputation.

John Arnot, æt. 8, was admitted on 1st December. His left knee was very much enlarged and immoveably bent at an acute angle with the thigh. There were two sinuses on the inner side of the joint, which allowed a probe to reach the bone. The disease resulted from a fall on the ice, and was of three years duration. His health was broken, and he seemed devoted to speedy destruction, unless something was done for his relief.

On Monday, therefore, 7th December, I made two incisions across the fore part of the joint, extending from one condyle of the femur to the other, meeting at their extremities, and including the patella between them. The integuments thus insulated being removed, together with the patella, which was very much diseased, I exposed the extremity of the femur and sawed it off. In doing this the periosteum was separated from the bone, to which it adhered very slightly, for about an inch or rather more, and I therefore thought it right to saw off another portion to this extent. The head of the tibia was next exposed, and removed by means of cutting pliers. One of the articular arteries was tied, and we then proceeded to dress the wound; but here an unexpected difficulty occurred, owing to the hamstring muscles, which, as already stated, were much contracted, and still prevented the limb from being straightened, notwithstanding the relaxation they had suffered in consequence of the removal of the joint. I extended the limb as far as was practicable, and secured it in this position by a splint and bandage. The patient had very little constitutional disturbance, but the wound presented a dry and unpromising appearance. The tibia, from not resting in opposition to the femur, was drawn up behind that bone, distended the integuments, and threatened to exfoliate extensively. I at length succeeded, by cautious extension and counter-extension, in reducing the displaced extremities of the bones, when the limb became quite straight, and the tendency to dislocation almost entirely ceased. The cure afterwards advanced satisfactorily, notwithstanding the most vexatious opposition on the part of the patient, who was a boy of uncommon quickness, but most perverse disposition.

Four weeks after the operation, the wound was all but healed, and the limb is now becoming every day more useful to the patient, who can already make considerable use of it in walking, though not yet provided with a high-heeled shoe.

Ann Mackintosh, æt. 7, entered the Hospital 14th December, on account of a white swelling of the right knee, which had existed eighteen months, and was now in its last stage. There was a large sinus above the inner condyle, through which I introduced my finger into the joint, and felt it extensively diseased. Encouraged by the success of the former case, I performed a similar operation in this one on the 28th December, and think it unnecessary to mention the particulars, as they were in all respects similar to those already detailed. The articulating portions of bone removed were, as in the boy, extensively ulcerated and carious; but the soft parts were much less swelled, and less altered by the gelatinous degeneration of scrofulous action than in him; the result, therefore, was expected to be, if possible, still more satisfactory.

Great difficulty was experienced, from contraction of the hamstrings, in preventing dislocation of the bones, and the femur, so far as it was visible, presented a bare and dead-like surface, but the favourable termination of the first operation, notwithstanding appearances equally disagreeable, prevented me from abandoning my sanguine expectations of success in this instance also. On the 6th of January, in order to prevent displacement of the bones, which all our efforts hitherto had been unable to effect completely, I cut away about two inches of the femur with the cutting pliers, and then observed, to my extreme concern, that the bone was denuded beyond the farthest extent to which my finger could reach. Amputation now seemed to afford the only chance; but, before having recourse to it, I resolved to wait a little, in the expectation of nature pointing out at what part of the limb the operation ought to be performed. On the morning of the 8th, I found her very weak; she sunk rapidly; and died at two the same day.

I do not think that the enemies of excision of the knee-joint can found anything upon this case, since it would appear from reasoning, and has been in great measure proved by experience, that excision of a joint is less dangerous than amputation of the limb; and the only question that can be agitated in respect to the merits of the operation in this situation concerns the utility of the limb which is preserved.

Exfoliation from the Pelvis.—In a late number of the Edinburgh Medical and Surgical Journal, I have put on record four cases of sinuses in the neighbourhood of the pelvis, which had been regarded as incurable, but which speedily healed after the extraction of small exfoliations of bone. One of these patients had been ill for two, another for five, and a third for seven years. The case of James Ormiston, æt. 17, was another of this kind, and tends to support the opinion expressed in the paper referred to, that many of the usually reputed incurable sinuses in the hip and upper part of the thigh are owing to the cause just mentioned. About thirteen months previous to admission, he received a blow on the hip with a stick from one of his companions, which caused discoloration, but no great inconvenience at the time. The part continued a little painful for six months, when it began to swell and form an abscess, which went on increasing to the time of admission. It was accompanied with great pain and stiffness when he bent his body forwards. I made an opening rather behind the trochanter major, for the matter, which was deeply seated. A large quantity of pus was evacuated with much relief; and a few days afterwards his dresser observed a piece of bone projecting at the orifice, which he removed. It was nearly an inch long. The boy now felt comparatively well, but the sinus continued to discharge; and there being considerable swelling, with pain and tension about the trochanter, I apprehended that more mischief was going on, and enlarged the opening, so as to admit my finger, which is the best instrument for exploring such passages. I then found that the trochanter was quite sound, and ascertained that the sinus ran deep between the muscles in the direction of the pelvis. The uneasy symptoms immediately disappeared, no doubt owing to the free drain which was afforded to the matter; and the boy was dismissed cured on the 21st of December.

Exfoliation from the Ankle.—John Rogers, æt. 9, Corstorphine, was admitted 4th January, on account of a disease of the ankle, which had been caused by a wound inflicted five months previously. The prong of a grape entered on the forepart of the malleolus externus, and passing obliquely across the foot, under the flexor tendons of the ankle, made its exit over the navicular bone. The wound healed in a few days, but the pain did not subside. Two months afterwards the ankle began to swell, and an abscess formed over the inner side of the joint, which was opened a fortnight previous to his admission. I found a piece of bone presenting at the orifice, and extracted an exfoliation of considerable size, fully an inch long, and half an inch broad. A few days afterwards one or two small pieces of bone came away, but the ankle still continuing swelled, stiff, and painful, I introduced a probe, and felt the bone extensively diseased. As the larger exfoliation seemed to have formed a part of the navicular bone, and as the ankle-joint, properly speaking, did not seem to be engaged in the disease, I proposed to perform a partial amputation of the foot through the tarsus, as in the case of Anne Stewart, which is related in the first Report, but the friends were adverse to any operation, and removed the boy to the country.

Fistula in Ano.—James Miller, æt. 29, admitted on the 22d September, had suffered from fistula in ano for nine years, and had hitherto been prevented from seeking relief by an exaggerated idea of the pain and danger attending the necessary operation. I found two fistulas, one communicating with the gut, the other not. I laid them both open, and dismissed the patient cured on the 16th of October.

James Preston, a middle-aged man, had suffered for two years from the usual symptoms of fistula, which were attributed by him to hemorrhoids. I found a fistulous opening into the gut, about an inch from the orifice, and laid it open. The patient was dismissed cured in a fortnight.

I cannot refrain from once more stating three important facts which concern the treatment of fistula in ano, *1st*, That the internal opening is rarely more than an inch distant from the verge of the anus. *2dly*, That the cure requires no more than a division of the parts between the internal and external openings. *3dly*, That the interposition of any foreign substance between the edges of the wound after the first twenty-four hours is not only useless but injurious. I could mention in illustration, some most striking cases which have occurred in my private practice since the date of my last Report.

Stricture of the Rectum, with fistula in ano opening into the vagina.—Robina Wright, admitted January 7th, three years ago began to suffer from pain in going to stool, with frequent desire to do so, and copious slimy discharge. She was then a servant in town, and up to that time had enjoyed good health.

To obtain relief from the complaints just mentioned, she entered the Royal Infirmary, and, with the exception of one week, remained there ever since, (three years,) as a physician's patient.

She complained of excruciating pain on going to stool, with an almost incessant call to do so. There was a copious discharge of slimy matter from the rectum, but its solid contents were never passed of a larger size than that of a quill. To relieve her distress she had been in the custom of taking three or four grains of opium daily, with a proportional quantity of the same introduced into the rectum. I found on examination a stricture of the rectum three inches from the orifice, so tight that the point of my finger could not pass through it. There was also a wide fistulous canal leading from the rectum into the vagina, and opening just within the orifice of each. Taking into account the youth of the patient, and the absence of any induration or thickening of the coats of the gut, I readily consented to attempt her cure.

I began by removing the stricture, which yielded without any difficulty to the introduction of steel bougies every third or fourth day. I then laid the fistula open, and she is now nearly well, being altogether free from pain in going to stool, and having no particular frequency of desire to do so. The amount of relief experienced in this case can hardly be conceived by any one who had not an opportunity of seeing the patient. The agonizing pain she constantly indured was plainly depicted in her countenance, and the fetid matter incessantly escaping through the fistula into the vagina, not only rendered her existence still more wretched, but made her an insufferable nuisance to others.

Lithotomy.—In the beginning of December Mr Moir asked me to see a boy, three years of age, who, ever since he was eight months old, had suffered the most excruciating distress in making water, which he nevertheless had almost incessant desire to do. His complaints had at first been attributed to the irritation of teething, then to that of worms; and about eight months previous to the time when I saw him he had been sounded for stone. The symptoms were now so strongly indicative of calculus in the bladder, that, though none had been detected on the former occasion, I thought it right to introduce a steel bougie, and very soon ascertained the presence of a stone. In performing the operation I encountered a difficulty which is frequently met with in young subjects, but rarely, I believe, to such an extent,—I mean a prolapsus of the gut. The intestine was almost constantly protruded; but when the child suffered from the irritation of making water, or the introduction of a staff, it descended so as to form a tumour not less than a goose's egg, which occupied all the space between the tuberosities of the ischium, and seemed to leave no room for performing the operation.

Finding it impossible to keep the intestine reduced, I held it as far as possible aside, and cut very carefully, so as to avoid any injury of it on the one hand, or of the pudic artery on the other. The difficulty was by no means so great as I anticipated, and I readily extracted, in the course of a minute or two, an oval calculus about the size of an almond with the shell. To prevent any risk from infiltration of urine, I introduced a caoutchouc tube. The boy never had a bad symptom, and made all his water through the urethra on the tenth day.

Angus Sinclair, æt. 77, presented himself at the Hospital 21st December, labouring under symptoms which led me immediately to sound him, when I readily discovered the presence of a stone. As the patient, though old, was not particularly weak, or otherwise unfavourably disposed for an operation, I performed it on the 23d. Having extracted one small stone, I was led to suspect the existence of others by its angular shape, and after some search detected five more; a tube being then placed in the wound, the patient was conveyed to bed. He seemed to suffer very little during the operation, which would have been completed in considerably less than a minute if it had not been for the number of stones.

The urine flowed freely from the tube, which was withdrawn two days after the operation; and the patient made no complaint until the 26th, when he became somewhat feverish, and altered in his appearance. There was not the slightest tenderness in the hypogastric region; and I flattered myself that the constitutional disturbance would subside as it generally does after great operations; but the patient became weaker and weaker, and died the day following.

Andrew Irvine, æt. 14, was sent up to the Surgical Hospital, from Lerwick in Shetland, by Mr Cowie, on account of symptoms so strongly indicative of stone in the bladder, that, though none could be felt on repeated examination, he was still believed to labour under this disease. I introduced a small sound, and discovered a calculus; and here I may take the liberty of remarking, that the instrument used for sounding is generally much too large, since, unless of a small size, it is grasped by the irritated parts surrounding the canal, so tightly, that it cannot traverse the bladder with freedom.

The boy stated that his complaints commenced when he was three years of age, and had continued ever since, though only occasionally so severe as to interfere seriously with his comfort. I performed the operation, and extracted a round shaped stone, about the size of a small walnut or large nutmeg, apparently composed of uric acid. The tube was introduced as usual. There was never the slightest tendency to any unpleasant symptoms. The urine resumed its natural course on the tenth day; and, on the fourteenth, he was perfectly well, with the exception of a small superficial sore, which resulted from the displacement of the edges of the wound.

The operation for lithotomy, as now performed, is one of the simplest in surgery; and the importance which is still attributed to it by the public, depends upon the recollection of the shocking and protracted tortures which attended the old method of operating with the gorget. The patient above-mentioned is the only one I ever lost from the operation; and his death, I think, may be ascribed fully as much to old age as to the injury inflicted.

Stricture of the Urethra, with Fistula in Perinæo.—Robert Mitchell, æt. 48, cooper, applied on account of a fistulous opening close by the verge of the anus, through which most of his urine passed.

I found a stricture at the bulb of the urethra, and had nearly cured it by the introduction of steel bougies every third or fourth day, so that he hardly passed a drop of water by the fistulous opening, when he unfortunately caught cold, and suffered a complete retention of urine. In these circumstances, I admitted him into the Hospital on the 13th October, and dismissed him cured on the 9th November.

Stricture of the Urethra.—Andrew Monro, æt. 30, smith, applied to me on the 10th of December, labouring under a complete retention of urine owing to stricture, from which he had suffered for three years. He was admitted and cured of his complaint, which was situated at the bulb, by the means abovementioned.

R. Clyde, æt. 28, sailor, applied for the cure of a stricture at the bulb, twelve months old. The cure proved a difficult one, but at length yielded so far to the usual measures, that a pretty large bougie could be passed. He then became an out-patient, and unfortunately was so well satisfied with the relief afforded by his imperfect cure, that he absented himself before it was completed. In all ranks of life, patients labouring under stricture of the urethra are apt to annoy their medical attendants in this way, since it is difficult to convince them, that unless the canal be dilated to its full size, the disease, though temporarily alleviated, will certainly return.

George Cockburn, æt. 31, applied on account of complete retention, owing to stricture, which had existed for five years. He made some objections to the introduction of a catheter, which he said was of no use, since attempts of the kind had been repeatedly tried, without success, by several most respectable surgeons in Newcastle, whence he had lately come to Edinburgh. Being thus prepared to meet with difficulty, I selected a very small silver catheter, and passed it at once into the bladder, to the patient's no small surprise and very great relief. I am now curing the stricture.

Catarrh of the Bladder.—Neil Ranken, æt. 32, from Lochaber, was admitted into the Hospital on 25th January, on the recommendation of Mr Smith of Badenoch. He complained of frequent, indeed almost incessant, desire to make water, with much pain in doing so. The urine was very turbid, and loaded with mucus, which adhered to the bottom of the urinal. He was easier in the horizontal posture than when erect and taking exercise. His countenance was anxious and expressive of severe and long-continued suffering. He stated that his complaints commenced without any obvious cause, and had existed for six months. The symptoms led me to suspect chronic inflammation of the mucous membrane of the bladder; but, to ascertain the truth more positively, I carefully examined the urethra, bladder, and prostate, in none of which any disease being detected, except a slight stricture near the neck of the glans, there remained no doubt as to the nature of the malady.

He has been taking small doses of copaiva, soda, and rhubarb, and occasionally using anodyne injections into the rectum, with marked good effect. I have no faith in the *vesicæ lotura,* or any of the means recommended for the cure of this disease, except those already mentioned, with local bleeding, blistering, and attention to the general health.

Diseased Prostate.—Alexander M'Lauchlan, æt. 63, admitted on the 10th of November, was recommended to the Hospital by Mr Forrest of Stirling, on account of severe urinary complaints, which had distressed him for two years. He had frequent desire to make water, and suffered great pain in passing it. The genital organs exhibited the conformation which is termed hypospadias. In the proper situation of the orifice of the urethra there was an opening which led to a short and narrow cul-de-sac, while the urethra terminated completely behind the neck of the glans by an extremely small aperture, through which a catheter could not be passed without previous dilatation. The urine drawn off was extremely fetid. The malformation just described had existed since birth, and consequently could not account for his complaints, which, as already mentioned, were only of two years duration. The existence of a stone had been strongly suspected; but on a most careful examination I could not find one, and therefore directed my attention to the prostate, which, when examined by the rectum, was ascertained to be considerably enlarged. In these circumstances, the patient was advised to return home and use the means which tend to palliate the miserable disease under which he laboured.

It is worthy of notice that this man's wife had four children.

Tumours of the Vagina.—Catherine Scott, æt. 20, was admitted on the 22d September on account of two pendulous tumours about the size of a small egg, which grew from the internal labia. They had existed four months, and were attended with much excoriation and uneasiness.

The tumours were cut off next day by means of scissors; the hemorrhage was restrained by compresses of lint; and she was discharged cured on the 1st of January.

The tumours when examined appeared to be merely extensions of the mucous membrane, and consisted of loose cellular substance distended with serous fluid.

Hematocele.—John Dunn, æt. 28, was admitted on the 26th of October on account of what seemed to be a hydrocele of the right tunica vaginalis. The fluctuation was distinct. There was no pain on pressure except at the back part; and though no translucency could be discovered, there seemed little room for doubting as to the nature of the complaint, especially as the patient stated, that during the twelve months which had elapsed since the swelling commenced, it had been tapped repeatedly, and found to contain a clear colourless fluid. On the last of these occasions, which was about three weeks previous to admission, the tumour regained its former size almost immediately after the operation, which was attended with very severe pain. Being dismissed from the Hospital where this happened, he now wished, if possible, to be radically cured of his complaint.

I introduced a trocar with the intention of drawing off the water and injecting the bag, but was surprised to see a dark chocolate-coloured fluid escape, instead of the serum, which had been expected, and that it did not exceed two or three ounces in quantity, while great part of the tumour, which had been nearly equal in size to a goose's egg, still remained. I now discovered, on careful examination, that the testicle was little enlarged; that the tunica vaginalis was very much thickened; and that the bulk of the swelling depended upon a soft mass which broke under the fingers like a coagulum of blood.

Concluding that the contents of the tunica vaginalis proceeded from blood effused into it, owing either to the wound formerly inflicted, or to a morbid action of its vessels, I saw no way of relieving the patient except performing the old operation of excision. I accordingly made a longitudinal incision about three inches long, evacuated a large quantity of coagulated blood, and, finding the membrane thickened fully to the extent of a quarter of an inch, very soft and dark coloured, I cut it all away, excepting where immediately investing the testicle. The cavity was loosely filled with caddis, but a pretty profuse hemorrhage having occurred in the course of an hour or two, I filled it very carefully with graduated compresses secured by means of a bandage.

A moderate degree of inflammation followed the operation. The caddis was taken out by degrees, and the diseased membrane that had been left, sloughed away, after which the cavity gradually contracted, and the patient, who had suffered hardly any constitutional disturbance, was dismissed cured on the 30th of November.

Hydrocele.—Of the cases of this disease which occurred, the two following seem to deserve particular notice.

William Paul, æt. 60, from Leith, was admitted on the 16th November, on account of a large hydrocele on the right side of the scrotum. It had existed about thirty years, had become more troublesome from its size twelve years ago, and within the last year had been tapped several times.

With the view of performing the radical cure, I introduced a trocar at the usual place, viz. on the anterior surface, about a third from the bottom of the swelling. Having drawn off a considerable quantity of fluid, I was surprised to find that nearly half the tumour still remained. The lower part had subsided so that the testicle could be felt all round, but the upper portion still existed as before. Having recognized fluctuation, and ascertained that there was no hernial protrusion through the external ring, I was satisfied that there must be two hydroceles, one of the tunica vaginalis, and another of the cord. Having injected the bag already evacuated, and allowed the port wine, diluted with an equal part of water, to remain four or five minutes, I punctured the other swelling, and having drawn off its contents, which were clear and colourless, I threw in some wine and water, which was retained but a very short time, as the history of similar cases tends to show that the disease is not so apt to return in the upper as in the lower swelling. The subsequent irritation was rather within proper bounds, but nevertheless proved sufficient for effecting a cure, so far as the hydrocele of the tunica vaginalis was concerned; and the patient was dismissed on the 14th of January, labouring only under the swelling of the cord. He returned a few days ago anxious to have it also cured; but as there was unfortunately no room in the house for his reception at the time, I was obliged to postpone the operation.

John Bryce, æt. 42, weaver, from Carnwath, was admitted on the 1st of February, on account of a moderately sized and distinctly marked hydrocele, which had existed for seven months.

As the patient seemed to be an irritable subject, I threw in but a small quantity of wine and water, and retained it for little more than a minute, remarking to the gentlemen present that they ought to be guided as to the time by the constitution and feelings of the patient, rather than by their watches.

On the day following the operation, the scrotum was very much swelled and red. Next day these symptoms were aggravated, and a slight bluish tinge was observed in the centre. On the third day the whole anterior surface of the swelling was black. I made a free incision through this part, and directed warm fomentations to be applied to the scrotum, and four grains of calomel with one of opium to be given twice during the day. Next day there was a line of demarcation between the sphacelated and living parts. The patient had enjoyed some sleep, and felt in all respects much better. I directed the resinous ointment with turpentine to be applied on caddis, and covered with a poultice. The slough has now separated.

This case is interesting as an instance of the violent effects which proceed from slight causes of irritation in peculiar constitutions. As a contrast with it, I may mention that of a gentleman, in whom I unfortunately injected the cellular substance of the scrotum instead of the tunica vaginalis. I converted the puncture into an incision about an inch long, and squeezed out some of the wine, but much of it still remained, and I prepared for violent local and constitutional disturbance, when to my surprise every thing went on favourably, and the patient obtained a radical cure speedily and satisfactorily.

Cancer of the Scrotum.—William Armstrong, æt. 32, mason, admitted 27th January, on account of an ulcer, about the size of a shilling, at the bottom of the scrotum, which presented a very obstinate and malignant appearance. The edges were excavated, thick and hard; the surface was smooth and discoloured; the discharge thin and fœtid; and there was much surrounding induration, extending to, but apparently not implicating, the testicle. The ulcer followed a hard tubercle, which was first noticed four months ago. On the 28th, I excised the ulcer, together with the surrounding induration, and found it necessary, in order to eradicate the evil, to shave off a thin slice of the testicle. The patient suffered no disturbance either local or general, and is now well.

Hernia.—On Sunday the 27th of December, I was asked by Dr Alison to meet Dr Ballingall in consultation on the case of M. G., an unmarried female, æt. 40, who, since the preceding Friday, had laboured under a strangulated femoral hernia, which had resisted all the ordinary means of reduction. The tumour was very large for a femoral hernia, exceeding that of a full sized orange. It was of old standing, but had never before attained such a size. Dr Ballingall and I having satisfied ourselves that the reduction could not be accomplished without an operation, recommended its immediate performance. The patient having no objections, was conveyed to the Surgical Hospital, where I operated soon after her arrival in the presence of Drs Alison and Ballingall.

The sac contained several ounces of serous effusion, a web of thickened omentum, and a fold of the small intestine. Having relieved the stricture, which was extremely tight, by cutting towards the pubes, so as to divide the crescentic portion of the crural arch, I proceeded to reduce the intestine, which was effected with some difficulty, owing to the distended state of the abdomen, and the size of the protruded part. I then cut away the thickened omentum, and dressed the wound.

The patient felt comfortable after the operation, which was performed at 4 P. M. Her vomiting ceased, and she had an evacuation from her bowels after an injection. During the night inflammation came on; and, notwithstanding bleeding, leeching, warm fomentations, &c. proved fatal on the succeeding evening at five o'clock. At the dissection, when Drs Alison, Graham, and Ballingall were present, we found the ordinary appearances which result from acute peritoneal inflammation of short duration. The gut which had been strangulated was a portion of the ileum, just before its entrance into the colon; its coats were much thickened and hardened; it was very dark in colour, and still retained the form into which it had been compressed while subjected to the strangulation.

In accounting for the unfortunate termination of this, as it seemed very favourable case, considering the age of the patient, her extreme composure of mind, and the absence of any symptom indicating inflammation in the hernial or abdominal contents, I have nothing to suggest but the peculiarities which are observed in the susceptibility of different individuals for peritonæal inflammation, and the rapidity and severity of its course. I recollect of operating last summer on two cases of femoral hernia, within a week or two of each other, where the result was very different from what might have been anticipated from their respective circumstances. One of these was that of a lady, in whom the strangulation had not existed quite two days, and that with occasional remission of the symptoms. The intestine was little altered from its usual state, and the patient's age, constitution, &c. promised the most satisfactory result. Nevertheless, peritonæal inflammation succeeded, and, after having its first acute attack subdued, recurred again and again in the most obstinate and alarming chronic form, so that, unless for the vigilance and skill of Dr Abercrombie, I am convinced it would have proved fatal. The other was that of the mistress of a lodging-house in the New Town, who had laboured under a strangulated femoral hernia for more than a week. Upon opening the sac, I found a transparent gelatinous mass, through which the gut, covered with coagulated lymph, appeared, if I might use the expression, like a potted eel. The coats of the intestine felt hard and leathery;—in short, all the circumstances seemed so unfavourable, that, on leaving the patient, Dr Abercrombie, who had been asked to see her at the same time with me, expressed his conviction that the result must necessarily be fatal, in which opinion I entirely agreed. Yet, strange to say, the operation was not followed by a single disagreeable symptom.

When speaking of femoral hernia, I may mention that I once operated for this complaint on a man. He was a soldier, a patient in the hospital at Leith Fort, under the care of Dr Mackintosh, and recovered perfectly; indeed the wound, as has often happened with me in operations both for inguinal and femoral hernia, healed by the first intention.

Amputation of the Thigh.—In the beginning of December, I was asked to visit Margaret Syme, æt. 11, on account of a swelling of the left thigh. The swelling was situated just above the knee, on the inner side of the limb; it was of an oval figure, about four inches long, and conveyed so distinct a feeling of fluctuation that I did not hesitate to make an opening with the view of evacuating its contents. To my surprise nothing but blood escaped, when, thinking that it was possible the incision had not been deep enough, I introduced a probe, and passed it readily to the bone, which was bare and rough. The nature of the disease was now sufficiently evident, and I next inquired into its origin and progress. The patient, who was extremely thin, with that greenish yellow complexion which is so frequently associated with medullary-sarcomatous growths, had for several months complained of severe aching pain in the bone. About two months previous to the time I saw her, the swelling was first observed, and had gradually increased ever since. The patient suffered severe and incessant pain. She had lost her sleep and appetite, and was evidently hastening to her end. In these circumstances, I recommended amputation, as affording the only chance of recovery, and with this view she entered the Hospital on the 15th December.

On the 17th, I amputated the thigh by two lateral flaps, and observed nothing particular during the operation, excepting that the bone was much more dense than usual. For several days the case seemed to be doing well, as far as could be judged from the local appearances and the constitutional symptoms. The wound, however, did not unite by the first intention; it suppurated profusely, and an exfoliation from the femur began to be detached. About three weeks after the operation, the patient became affected with diarrhœa and other symptoms of hectic, which, notwithstanding all our care, continued to increase until they at last proved fatal on the 10th of January. The tumour when dissected exhibited a most beautiful specimen of medullary sarcoma, being composed of a bloody brain-like mass lying between the periosteum and bone.

Catharine Wallace, æt. 28, applied at the Hospital on the 10th of January, on account of a swelling of the wrist, which she attributed to a fall sustained five months previously. The tumour, which occupied both aspects of the limb, was soft and fluctuating. She blistered it repeatedly without any good effect, and I then, in the belief that it depended on a deep-seated collection of matter, made a free incision, from which there issued nothing but a little blood. The wound healed by the first intention, and the patient even fancied that it had afforded her some relief, but the swelling still continues, and will soon, I fear, require amputation of the arm.

Amputation of the Arm.—Margaret Donaldson, æt. 65, was admitted on the 13th of November, for a disease of the elbow, which seemed to require amputation, both on account of the local distress occasioned by it, and the constitutional disturbance resulting from its presence. There were two sinuses opening on the inner side of the limb, which extended to the bone, and constantly poured out a copious discharge of matter. About three years ago I amputated the arm, between the wrist and the elbow, on account of a most extensive caries of the wrist, soon after which time her present complaint commenced and has continued ever since, with occasional exacerbations and remissions. She wished to have the arm removed; but I was prevented from performing the operation, for ten days after her admission, by repeated attacks of erysipelas and constitutional disturbance. At last, on the 23d November, I amputated the arm by double flap. She recovered perfectly without any remarkable occurrence, except occasional attacks of the erysipelatous affection, and was dismissed cured on the 19th of December.

Mr Samuel Cooper, in his Surgical Dictionary, has alleged, in objecting to my arguments in favour of the flap mode of amputation, that I was not aware of the fact that muscles, when not exercised, suffer a degeneration which deprives them more or less completely of the characters which distinguish the muscular tissue. I never supposed that the muscular parts of a stump would continue as full and plump as they were previous to the operation, but believed, that, though somewhat attenuated, yet being approximated to a tendinous consistence, they would still more effectually fortify the stump against external injury, than if they had remained in their original state In examining the stump removed in this case, I was rather surprised to find the muscles which covered the extremities of the bones still presenting their natural characters.

Amputation of the Leg.—George Robertson, æt. 10, entered the Hospital on the 24th of September, on account of caries in the articulation of the astragalus and os calcis. As the disease, which had existed for eleven months, was seriously affecting the patient's health, I recommended amputation, and performed it at the middle of the leg by single flap on the 24th of November. The patient recovered without any trouble, and was dismissed cured on the 26th of December.

Mr G. applied to me on the 17th of December, on account of a disease in the foot, which had existed upwards of seven years, for the last three or four of which he had been unable to use it in walking. The foot was swelled, stiff and painful about the tarsus and ankle-joint, and, when moved, occasioned an obscure crepitation. All sorts of remedies, such as leeches and blisters, iodine and mercurial ointments, with pressure, had been used without benefit, and the patient's patience was now so completely exhausted by suffering and confinement, that he was desirous of having the limb removed. Though I should by no means have felt inclined to recommend an operation in such circumstances, I felt unwilling to refuse compliance with the patient's desire, as there seemed little reason to expect any amendment in the leg. He was not reduced by disease, and was not suffering much irritation from it, but he was of a cold phlegmatic temperament, which made me less apprehensive of danger than I should otherwise have been in the same circumstances.

I amputated the leg mid-way between the knee and ankle by a single flap on the 21st of December. The patient was hot and restless after the operation, and next day became suddenly delirious, tossing about the stump, and insisting upon getting out of bed. I bled him to sixteen ounces, and prescribed a purgative injection, to be followed by repeated doses of laudanum and antimonial wine. He became quite composed in the evening, but passed a restless night. Next day he continued in the same state, with frequent cold shivering fits. He took senna, Epsom salts, and tartrate of antimony. Next day he was much better, and the wound continued as it had done all along, to present the most satisfactory appearance. On the fifth and sixth days he was not so well, being restless and frequently affected with shivering fits; his ideas wandered; his countenance had an unmeaning expression; and the skin all over the body had acquired a yellowish tinge. On the seventh, all the symptoms which have been mentioned were considerably aggravated, and on the eighth day he died, with all the appearance of a person labouring under the most confirmed jaundice.

Every practitioner must have met with cases similar to the one just detailed; and I am at a loss to suggest any means by which the fatal result could have been prevented, unless perhaps a longer and more careful preparation for the operation than was thought necessary, as already mentioned. In operating upon patients who have not been reduced by their diseases, or are not suffering much irritation from them, I have always been accustomed previously to lessen the strength of the system, and accustom the individual to confinement in bed. For instance, a short time before operating on the case under consideration, I was asked to extirpate the testicle of a gentleman, who came from the country for that purpose. As he was remarkably stout, I advised him immediately to go to bed, and take a succession of smart purgatives and diaphoretics, which, in the course of a few days, reduced him to a favourable state for suffering the operation.

Amputation of the Leg.—Thomas Gibson, æt. 11, applied on the 26th of December, on account of a diseased foot, which was with some difficulty exposed to view, owing to the complicated investure of the apparatus which has of late come into fashion for the treatment of diseased joints through the recommendation of Mr Scott. The bandages, plasters, ointments, &c. having been at last taken away, two sinuses made their appearance, into which I was about to introduce a probe, in order to ascertain the source of a purulent discharge, which seemed extremely profuse, when, holding the anterior part of the foot in one hand, and the heel in the other, I felt that the articulations in the middle of the tarsus were completely destroyed, so as to admit of motion in all directions. The disease had commenced sixteen months previously in consequence of a strain, and for the last eleven of these had been subjected to the system of pressure. As the boy's health was now much impaired, and the disease was most thoroughly hopeless, I recommended amputation; which, being readily agreed to by the parents, was performed rather below the middle of the leg by single flap on the 11th of January. The wound healed kindly; and the patient was dismissed cured on the 1st of February. It is much to be regretted that the excellent practice recommended by Mr Scott has become the cause of great and frequent mischief owing to its misapplication.

Amputation of the Great Toe.—William Bell, æt. 26, from Moffat, recommended by Mr Walker, was admitted into the Surgical Hospital on the 7th of October, on account of a diseased great toe, which had rendered him lame for two years. There were several openings over the joint, through which both phalanges could be felt very much diseased. And I had therefore no hesitation in amputating the toe at the metacarpal articulation. The wound healed kindly; and the patient was dismissed cured on the 24th of October.

Amputation of the Fingers.—Four fingers were amputated on account chiefly of whitlow.

Gun-shot wound of the Hand.—Walter Frazer, æt. 27, private of the 9th Lancers, being on a visit to his friends in the neighbourhood of Trinity, while shooting sea birds, happened to fall in passing through some brushwood, and in raising himself unfortunately placed the right thumb over the muzzle of his piece, at the same time that its contents were discharged by some of the twigs pulling the trigger. The distal phalanx was blown off quite smooth, with the exception of a small piece of the bone which was shattered into the joint. At first sight I thought of amputating the contused surface, but abandoned this intention at the suggestion of my friend, Dr Ballingall, who remarked, that his fitness for military duty would be very much affected by any farther diminution of the thumb. I therefore merely removed the broken fragments of the bone. For nearly a week the patient came daily to the Hospital from his father's house, about four miles off, to have the wound dressed, but he then began to complain of great pain in the wound, with diffuse inflammation of the fore-arm, and violent disturbance in the system. I took him into the Hospital on the 15th of January, and next day made an incision near the wrist, on the palmar side, through which a large quantity of matter was evacuated. A succession of abscesses formed in different parts of the hand, and the patient, through pain, want of sleep, diarrhœa, and profuse perspiration, became extremely reduced. His general health is now nearly restored, but the hand still continues œdematous and painful.

Extirpation of the Mamma.—Mrs Williamson, æt. 30, from Aberdeen, was admitted on the 24th of November, with well-marked scirrhus in the right mamma of seven years standing. The skin adhered in two parts to the diseased gland, and in the axilla several hard knots were perceptible. But notwithstanding these unfavourable circumstances, the youth and apparent good health of the patient induced me to comply with the almost unanimous recommendation of her former medical attendants, and I removed the breast with the indurated glands in the axilla on the 26th of November.

A very large triangular portion of integuments having been removed, it was impossible to bring the edges so closely into contact as to prevent a space the size of half-a-crown from remaining uncovered. With this exception, the whole of the large wound healed by the first intention, and the patient was dismissed cured on the 23d of December.

Omalgia.—Christian Mathison, æt. 30, was admitted on the 12th of January, on account of a severe pain originating in the shoulder, but affecting the elbow and fingers so severely as to prevent her from sleeping, and totally unfit her for any employment. She had used leeching and blistering, and tartar emetic ointment without any benefit, and the complaint, which had existed for five weeks, was becoming every day worse and worse. I proposed the actual cautery, and, meeting with no objections, applied it next day over the posterior part of the joint. The patient almost immediately experienced relief, and in the course of a few days was completely well, with the exception of the ulcer which resulted from the burn.

Partial Paralysis of the Fore-Arm.—There were four cases very similar to each other of palsy of the extensors of the hand. The patients could not straighten any of the joints from the wrist downwards, nor supinate the hand, but in other respects retained its use. They were all cured by repeated blistering over the extensor muscles, in a period varying from one to three weeks.

Tumour of the Face.—Mary Grieve, æt. 50, from the neighbourhood of Dunbar, was admitted 14th of August, on account of a tumour about the size of a walnut, seated at the inner angle of the eye. It was ulcerated on the surface, and had a soft consistence. I excised it on the 15th of August without any difficulty, and the wound, which was of considerable size, owing to the broad basis of the swelling, speedily contracted, so that she was dismissed cured on the 5th of September.

The tumour when divided presented all the characters of medullary sarcoma.

It is an interesting fact in this woman's case, that her breast was cut out eighteen years ago by Mr John Bell on account of scirrhus.

Tumour on a Child's Head.—On the 20th December, Mr George White asked me to see a male child, six months old, apparently in good general health. On the back part of the head, on the left side, behind the ear, there was a round, flat tumour, about two and a-half inches in diameter. It had somewhat of the consistence of a fatty tumour, but adhered to the bone. The mother observed the tumour soon after birth, since when it had been constantly increasing, and was now doing so more rapidly. She had showed it to several practitioners in town, who declined interfering, alleging that the child was too young to bear an operation, or that the tumour could not be removed. Having examined the swelling very carefully, and satisfied myself that there was no pulsation, I recommended an immediate operation, since there could be little doubt, that, if the tumour continued to increase at the rate it was then doing, extirpation would soon be impracticable. Dr Ballingall and Mr White being of the same opinion, the operation was performed, and no difficulty occurred except a pretty firm adhesion of fully half an inch in extent to the bone, which was at this part extremely thin, so as to yield readily upon slight pressure. It was in the centre of this space that the principal nutrient artery of the tumour issued from the bone. I applied a ligature here, and then dressed the wound with compresses of lint and a bandage. During the succeeding night considerable hemorrhage occurred owing to the shifting of the bandage, and was arrested by its more careful application. The child did well afterwards, and the wound healed. The tumour, when cut across, exhibited a very perfect and beautiful specimen of the cystic sarcomatous structure, consisting of numerous independent cells, which were filled with thin fluid of various colour, from light yellow to dark red.

Excision of the Tonsils.—Catharine Cunningham, æt. 24, a servant in town, was admitted on 26th August on account of sore throat and deafness. On examination I discovered a very large swelling of the tonsils, which sufficiently accounted for both her complaints. I cut away the projecting portion on both sides by means of a hook and scissors, immediately after which she recovered her hearing, and ceased to be troubled with the uneasy symptoms in her throat.

Chronic enlargement of the tonsil is a very common disease in cold moist countries such as this, and the inconvenience which proceeds from it is no less complicated than distressing. The faculties of speech, smelling, tasting, and hearing, are all more or less affected, besides the constant uneasiness which is caused by the presence of a hard swelling in the throat, and the frequently recurring annoyance of inflammation in the morbid growth. It is therefore a fortunate circumstance that surgery affords a ready and certain mode of relief, viz. excision of the tumour. In doing this it is not necessary to remove the whole of the swelling, since the new actions, which are induced by the abstraction of a part of it, generally suffice for curing the remainder. The operation, therefore, is perfectly safe and easy; indeed, even granting that it were necessary to remove the whole of the tumour, the operation would still be attended with hardly any danger, since the carotid artery, which is the grand source of apprehension, runs parallel with the direction in which the incision ought to be made.

There was another patient affected with this complaint admitted into the Hospital to-day, whose case will be detailed hereafter.

Polypus Nasi.—The soft mucous or benign polypus, as it has been variously named, usually grows from the upper part of the nasal cavity, never, so far as I know, from the septum, and but rarely from the inferior spongy bones. When it occupies this last situation, the method of evulsion, which usually answers so well, cannot be practised with advantage, owing to the broad attachments of the swelling; and the best method of proceeding is to introduce a pair of scissors along the margin of the bone, and either completely detach the tumour, or, at all events, so weaken its connections, as to render extraction with the forceps a matter of no difficulty. The largest polypus of this sort I ever extracted was from a lady upwards of eighty. I have met with it much more frequently in children than in adults.

Helen Mackinnon, æt. 7, applied at the Hospital, August 28th, on account of a difficulty of breathing through the nostrils, which she had experienced since infancy, but which had become much more distressing within the last eighteen months, when a fleshy excrescence was perceived obstructing them. On examination, I discovered a mucous polypus in each nostril, growing from the inferior spongy bone, and easily removed them by first cutting the principal part of their connections with scissors, and then completing their extraction with forceps.

Brachial Aneurism.—William Gillon, æt. 20, was admitted on the 4th of February, on account of a pulsating tumour, the size of an egg, at the bend of the right arm, which had resulted from a venesection performed about a month previously. The following statement of the gentleman in whose hands this unfortunate accident occurred affords the best history of the case:—

"On the 11th January 1830, William Gillon complained of pain in the umbilical region, increased on pressure. He had diarrhœa, foul tongue, quick and full pulse. The median-basilic vein was opened while the patient sat upon a chair. The blood flowed steadily in a small stream, and had a dark colour. A thrombus formed, and before six ounces of blood had flowed the patient fainted. The arm was then bound up. A little castor-oil was given; and after its operation the patient felt almost quite well.

"On the 16th January, the arm in which the vein had been opened was found considerably swelled and ecchymosed, from the neighbourhood of the lancet-wound upwards, as far as the belly of the biceps muscle. The patient said he felt pain only over the superior part of the tendon of the biceps. In the course of three or four days, during which warm applications and frictions with camphor liniment were used, the swelling and ecchymosis disappeared, and the pain over the biceps subsided. On the 25th January, (fourteen days after venesection had been performed) the patient complained of a little pain at the bend of the arm. Upon examination, there was found, just under the cicatrix of the lancet-wound, a pulsating tumour of the size and form of a filbert. When the humeral artery was compressed, the pulsations in the tumour were arrested for a short time, but they always returned again, even when the same degree of pressure on the vessel was continued. Compression over the aneurism itself was therefore the only curative means employed. At first this promised to be successful, as the tumour diminished somewhat, but it afterwards increased again to its present size."

I am quite at a loss to account for the long period which intervened between the infliction of the wound which gave rise to this aneurism and its first appearance, and I am still more at a loss to account for the accident occurring in the hands of a gentleman who is one of the most cautious and well-informed practitioners with whom I have the pleasure of being acquainted. The fact of its having done so, ought to put every one on his guard in opening the median basilic, and make us charitable in judging those who happen to wound an artery.

I tied the humeral artery a few inches above the elbow on Friday the 5th of February. The arm immediately became numb, and the pulsations of its arteries ceased until the evening, when they returned both at the wrist and in the tumour. Since the operation he has not complained of any pain in the aneurism, which has become much smaller, less prominent, and much more feeble in its pulsation. I trust that this favourable change will continue and increase until a cure is obtained, but will state the result in next Report.

Attempted Suicide.—There were two patients admitted on account of attempts at self-destruction,—one a young woman, who swallowed two pennies worth of acetate of lead, the other a female advanced in life, who cut her throat. They both recovered.

10*th February* 1830.

Art. II.—*A Case of Retroverted Uterus, treated by Puncture of that Organ, with remarks on the circumstances which indicate the employment of that operation.* By J. M. Baynham, Surgeon to the General Dispensary and to the Town Infirmary of Birmingham.

THE consequences of retroversion of the uterus have been so often fatal, that a case successfully treated by surgical operation cannot be devoid of interest: The practice adopted, in this instance, will be found uncommon; and, since it led to a successful issue under the most unpromising circumstances, deserves to be recorded.

Hannah Martin, aged 30, of spare make, was admitted a patient of the dispensary, 28th of March 1828. She was then in the sixth month of her second pregnancy, the history of her case to which period is briefly as follows.

When employed six weeks previously in moving a heavy weight, she suddenly felt acute pain in the lower part of the belly. To this, however, little importance was attached at the moment. Two days afterwards retention of urine occurred, with almost constant pain. The nature of the case appears to have been overlooked by the gentleman consulted in the first instance, since the use of a catheter was not proposed. She had dripping of urine, with progressive enlargement of the abdomen during the next month; at the end of which time, finding no relief in medicine, she applied to another surgeon, who, by the introduction of a catheter, obtained eight pints of urine in the morning, and nearly the same quantity seven hours afterwards. No examination, *per vaginam*, was even now instituted, and, of course, no permanent relief secured to the patient, the catheter only being used night and morning in the next fortnight. When recommended to the dispensary she had kept her bed three weeks, and was in a state of high fever, her pulse 136, short and indistinct. She had frequent vomiting, constant micturition, tenesmus, fulness, tension, and tenderness of the abdomen. In my first attempts to pass a catheter, I was embarrassed by the altered state of the external organs, a large portion of the vagina being prolapsed, and the clitoris and nymphæ greatly enlarged. The urine which escaped by the instrument resembled the contents of a psoas abscess, but was much more fetid. The entire cavity of the pelvis was occupied by a tumour which caused protrusion of the anus, and also eversion of the lower extremity of the bowel. The mouth of the uterus was far beyond the reach of the finger, and the fundus of this organ was situated less than one inch from the anus,—a circumstance which rendered the admission of the finger into the rectum, a work of much difficulty. Feeling satisfied that no urine remained in the bladder, I attempted to replace the uterus by a gradual introduction of the whole hand into the vagina. The *os uteri* pointed directly upwards, and was raised above the pubis; in fact, the retroversion was complete.

Having persevered as long as seemed consistent with the safety of the patient, I requested the attendance of two of my colleagues, and they met me in consultation the same afternoon

(March 28). She had become much more exhausted and restless. Her anxiety of manner, and the failure of her pulse, leading us to suppose that she was nearly moribund, I proposed the immediate introduction of a trocar into the uterus for the purpose of lessening its volume. Preparatory to any other steps, the catheter was again used, and having then placed the woman upon her elbows and knees, I once more endeavoured to raise the tumour, but not succeeding better than before, I slowly passed my hand into the rectum, and, adapting it as far as possible to the base of the tumour, continued for some time to make the firmest pressure without sensible advantage.

Mr Blount, one of the gentlemen present, in the expectation of a better result, desired to satisfy himself of the impracticability of success before the operation of puncture was adopted. Having passed his finger into the *os uteri*, he endeavoured to rupture the membranes; but, although assisted by a curved metallic instrument, he was compelled to relinquish his purpose, and all other expedients to relieve the patient having failed, it was determined to employ the trocar. In this proceeding I selected the most prominent point of the tumour in the rectum. The entrance of the trocar not being followed by any discharge, it was withdrawn and introduced a second time in nearly the same situation. About twelve ounces of colourless fluid now escaped by the canula, but not without frequently changing its position; since the opening was at times obstructed by the presence of the child. The fulness of the uterus having thus been diminished, attempts were again made to carry it above the brim of the pelvis, and this was effected in less than a quarter of an hour. When the organ had recovered its proper situation, the *os uteri* was found partially dilated, and the membranes somewhat protruding. A full opiate was prescribed, and the woman passed a better night than any in the previous month.

The next morning, although still in a state of great exhaustion, she was decidedly improved. Labour pains occurred in the evening of the 29th, and less than one hour sufficed, fortunately without hæmorrhage, to exclude the contents of the uterus, twenty-five hours after the operation. The ovum was entire, the membranes perfect, and still retaining ten ounces of *liquor amnii* untinged with blood. The fœtus was perfectly fresh, and of the ordinary size at six months. The trocar both times had penetrated the substance of the placenta near to the insertion of the cord, and once had entered the abdomen of the child; forming an aperture through which nearly the whole of the small intestines were forcibly protruded by the pressure subsequently used. The second puncture was referable to this unavoidable accident. It is worthy of remark, that, notwithstanding the placenta was twice perforated, hardly a tea-spoonful of blood was lost.

The catheter was used but once after this time, when a pint of equally offensive urine was evacuated. Incontinence then supervened and lasted nearly five weeks; and severe pains continued to be felt in the pelvis for some time. Copious vaginal discharge, added to the *stillicidium urinæ,* kept up a state of soreness and excoriation; and it was not until after a month that her urine lost its fetor. Considerable masses of coagulated lymph were often discharged, and, at separate times, four pieces of regularly organized membrane, which were mistaken for portions of the bladder, but which subsequent events happily proved to be parts of the vagina only. At the end of April she had the satisfaction of holding small quantities of urine, and in a fortnight could retain it almost as well as before her illness. The rectum was longer in recovering its tone than the vagina; purulent evacuations taking place from the former passage, with frequent and sometimes distressing tenesmus, until after she was in other respects well. It is probable that an abscess formed in the cellular substance, between the vagina and rectum, since the matter voided *per anum* was different, and more in quantity than the mere surface of the bowel could have yielded. She kept her bed three weeks before she applied to the dispensary, and did not leave it until nearly a month afterwards. Upon the 7th of May, she was sufficiently recovered to leave home and engage in her usual occupation. Menstruation occurred in the first week of June, and she has continued in good health since that period.

Dr William Hunter was the first to suggest an expedient, the merits of which have, since his day, been freely canvassed and somewhat freely censured. "Would it not," he asks, "be advisable in a bad case to perforate the uterus with a trocar, in order to discharge the *liquor amnii,* and thereby render the uterus so small as to admit of reduction?"* If other methods failed, he thought such an operation ought to be tried. It does not, however, appear that it has ever, until now, been attempted in this country, and not more than once upon the continent. The suggestion was reprobated by Dr Duncan soon after it was published, because "death has invariably followed punctures of the uterus wherever that happened accidentally, for it was probably never done intentionally."* Dr Cheston makes mention of a case wherein, by using a long trocar for the purpose of puncturing the bladder, this was transfixed, and the uterus unintentionally penetrated. The woman died, it is alleged, in consequence, but in all probability from infiltration of urine in the cellular substance. † By inflammation thus excited, Dr Cheston himself nearly lost a patient. He used a very short trocar, and as the bladder collapsed, permitted the instrument to slip out, and the urine to become effused. More recent experience proves that operations upon the womb do not subject patients to greater danger, or worse consequences, than are attendant upon wounds in many other parts. It is hardly necessary to advert to the issue of Cæsarean operations, as performed upon the Continent; or to cases of extirpation or partial removal of the morbid uterus. In confirmation of the safety with which a similar, although less extensive injury may be inflicted, I may adduce the late authority of Dr Blundell, who, for the purpose of inducing abortion in cases of extraordinary deformity of the pelvis, recommends a sort of acupunctural operation above the pubis, by which the ovum may be broken up, and an effectual stop put to the generative process.

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* Med. Obs. and Inquiries, Vol. iv. p. 106.

That the *pregnant uterus* may be punctured, *even without injury to the child*, is shown by Mr Simmons of Manchester, who was requested to tap a woman from whom he once before procured a large quantity of water. The trocar entered with difficulty, and nearly half a pint of blood being lost from the placenta, the instrument was withdrawn. She was suspected to be with child; but the operation was not performed sufficiently high to save the uterus. Fourteen weeks after this accident, she was delivered of a fine healthy girl, and subsequently tapped with success. ‡ I do not expect that no more fluid will in all cases be obtained than shall obviate the risk of abortion; but this is not impossible. The safety of the operation which, under circumstances of peculiar difficulty and danger, I am disposed to advocate, is placed beyond doubt; and although it is not to be supposed that the result of one or two experiments furnishes a rule of practice, it forms a ground of encouragement in cases otherwise hopeless; and in such only will it ever be adopted.

The extent to which the size of the tumour may be reduced by evacuation of the water of the amnion is considerable; since the respective quantity is much greater in the commencement than at a more advanced period of pregnancy. Sabatier admits the probability of success in reference to the mother, but, fearing that destruction of the child will almost necessarily follow, he gives the preference to puncture of the bladder, presuming that replacement of the womb will thence always become easy.[*] Now it is only to cases in which replacement shall be found impracticable, *after the bladder has been emptied*, that perforation of the uterus will apply. Sabatier did not calculate upon this exigency, although it is pointed out by many cases upon record. Although, in the case above related, the operation was performed by the rectum, I do not consider this an eligible situation. It was selected because the uterine tumour may be said to have pointed most distinctly in the bowel. Perforation of the uterus through the vagina is preferable, since, without an equivalent advantage, even this small wound of the intestine ought to be avoided. Moreover, there will be less probability of injuring the placenta, which is usually attached to the fundus; and, trifling as the chance may be of preserving the fœtus, it is entitled to consideration.

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* Medical Commentaries for 1794, p. 154.

† Medical Communications, 1790, Vol. ii.

‡ Med. Facts and Observations, Vol. viii. p. 27.

The uterus, which in the case above detailed had become displaced about the end of the fourth month of pregnancy, continued in a state of retroversion at least six weeks, during which time the developement of the ovum advanced, until it occupied the whole cavity of the pelvis, the child being fixed beneath the pubis and the angle of the sacrum. The extent to which an uterus may become immoveably packed in the pelvis is shown by Dr Hunter, who, in a post mortem examination, "found it in a retroverted state grown so large, and thence so wedged, that he could not take it out until he had cut through the symphysis of the ossa pubis, and torn those bones asunder to enlarge the space within the bones of the pelvis."[†] This unnatural position of the uterus does not materially, if in any degree, impede the growth of the child so long as the capacity of the pelvis exceeds the bulk of the uterus; and, before these proportions are likely to be reversed, a remedy must be applied, or the patient will be lost from mischief done to the bladder. The distension of the bladder (two gallons of urine having, by the use of the catheter, been obtained in six hours,) may seem incredible. A still larger quantity was drawn off by Dr Hunter, who, after retention had existed five days, procured at one time seven quarts, and nearly half that quantity in the afternoon of the same day. The further use of the instrument was impracticable, and after death the bladder was found amazingly distended, being as large as the womb in the ninth month of gestation, and filling the whole cavity of the abdomen.* Retention of urine is perhaps more frequently a cause than the effect of retroversion. This, however, when partial, will be rendered worse, if not complete, by distension of the bladder, which, as it increases in fulness, must of necessity rise more and more above the pubis, carrying with it the cervix, and proportionately depressing the fundus uteri. The evil is still further aggravated by the frequent and spasmodic efforts of the patient to relieve herself.

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[*] Médecine Opératoire, Tom. ii. p 483.

[†] Medical Obs. and Inquiries, Vol. iv. and for a similar instance, see Wilmer's Cases, p. 144.

There are but few derangements of the human body yielding so large a proportion of fatal cases as retroversion of the womb; an event almost uniformly produced by injury to the urinary bladder, which, upon dissection, exhibits the marks of inflammation, effusions of lymph, ulceration, mortification, and rupture; and yet by some the complaint has been ranked under the most innoxious class of female disorders. It has been stated as a guide to practice, "that the uterus will sustain no injury by this displacement; that there is no danger of its being locked in the pelvis; that it will frequently be restored by its own efforts to its natural position; and that neither difficulty or danger will occur, if the catheter be used once or twice daily." † I have witnessed the death of two patients from this cause, and the dangerous situation in which others have been placed. It is very true that an early and frequent use of the catheter affords relief, and sometimes proves a source of recovery; but though some writers upon this subject place much dependence upon the curative efficacy of simply keeping the bladder empty, it will in general be found expedient, immediately after the employment of the catheter, to effect reduction of the tumour by manual assistance; since it is only when *partial* displacement has happened that spontaneous restoration is likely to occur. The location of the tumour fairly beneath the brim of the pelvis, resting, as it must do in *complete* retroversion, within the hollow of the sacrum, seems to obviate the possibility of natural recovery. The mechanical impediment to reduction of the organ depends upon the undue elevation of its cervix; whereby the long diameter of the uterus is rendered more than commensurate with the short diameter of the pelvis.

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* Medical Obs. and Inquiries, Vol. iv.

† Dr James Bell, Med. Facts, Vol. vviii.—Denman's Introduction to the Practice of Midwifery.—Merriman's Treatise on Retroversion of the Uterus.—Burns' Principles of Midwifery.

The period of quickening which, under ordinary circumstances, carries the uterus into the abdomen, cannot here avail any thing. Perfect retroversion may be caused by accident; but accidental restoration is hardly to be expected. I do not imagine that mischief can be produced in any case by artificial aid carefully administered; whereas both difficulty and danger may ensue from too much reliance upon the efforts of nature. * Although it often happens that abortion will succeed the operation of causes apparently trivial, it will in general be found, that, even where violence has been used in restoring a displaced uterus to its proper situation, the woman will go her full time, and be delivered of a healthy child. The position in which the patient is usually recommended to be placed, upon her elbows and knees, cannot afford assistance, unless it enables the operator to direct his efforts with advantage to himself. The influence of gravitation is not to be dwelt upon; otherwise the obsolete practice of treating strangulated hernia ought to be continued; thus reversing not only the order of nature, but the lessons of experience. In some women there is a marked disposition to the recurrence of this accident, owing principally to a lax state of the soft parts, and a more than ordinary capacity of the pelvis. I have known it happen to a woman under thirty years of age in four successive pregnancies. Under such circumstances, no obstacle of moment will be opposed to replacement of the organ; it will be more difficult to secure the patient against a relapse; and for this purpose, it is obvious that, together with the use of gentle laxatives, and proper attention to the state of the bladder, the recumbent posture must be observed until after the ascent of the uterus.

Having adverted to a case upon the Continent, in which the operation of puncture was first executed, I am led to mention some of the particulars, notwithstanding their apparent inconsistencies.† The patient was attended by M. Jourel of Rouen. A woman æt. 23, the mother of one child, experienced the indications of a second pregnancy. Six weeks subsequently, after a frolic, in which she was suspended by her arms and legs, she had discharge from the vagina; pains in the loins and about the hips; a sense of weight in the perinæum; difficulty in walking, and in the evacuation of her stools. M. Jourel, who was consulted on the fifteenth day, advised rest.

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* Mr Weir, who has collected much information upon this subject, inculcates the same doctrine in a paper recently published.—Glasgow Med. Journ. August 1828.

† For a detailed account, the reader is referred to the Eighth Number of the Bulletin de la Faculté de Médecine de Paris for 1812.

The patient went into the country for a month, at the end of which time the discharge of blood had ceased only within the two last days. The other symptoms progressively increased, so that she could no longer pass her urine or stools without great difficulty. Six days afterwards, (September 13th,) a firm body, of which the large extremity compressed the rectum, and the lesser extremity the bladder, was found in the vagina. M. Jourel and his colleagues, suspecting it to be a retroverted uterus, endeavoured to replace it. Their endeavours were ineffectually repeated on the following day (14th,) the whole hand being passed into the rectum for the purpose. An attempt was also made to introduce a bent catheter into the mouth of the womb, and thus to rupture the membranes of the supposed ovum. The neck of the uterus, being much curved, opposed this expedient; which, whenever it can be accomplished, must take precedence of puncture through the uterus.

M. J. eventually passed a trocar, conducted along the vagina upon the fore-finger of his left hand, into the uterus. A *much larger quantity* of bloody water escaped by the canula than is usually collected at the same period of gestation; but which, says the author, the peculiar circumstances of retroversion may explain; "ubi stimulus ibi fluxus." The womb at once became soft, the pulse less frequent, and the general state of the patient ameliorated. She was, however, so much fatigued, that replacement of the tumour was deferred. From this moment her urine flowed easily, and she slept well. During the two following days but little change occurred. She had still a copious discharge of serum by the vagina. Upon the 17th, the pulse became frequent and feeble; the hypogastric region painful to the touch; the uterus firmer and more sensible. She again passed her urine with difficulty, vomited twice, and had more prostration of strength. The discharge ceased, but reappeared on the 18th, when she became the subject of diarrhœa, and at times of incontinence of urine. 22d, She had a grayish and putrid discharge *per vaginam*, and a copious escape of urine when raised in her bed. By the aid of tonics, her strength, which had been sinking rapidly, was repaired in a few days; and the fetid discharge occurred only at intervals. It was not before the 27th of the month that the neck of the uterus, hitherto curved, *spontaneously* resumed its place in the middle of the pelvis. October 2. (In little more than a fortnight,) the organ had recovered its natural size, and the discharge was much lessened; the pulse continued frequent; and towards evening the patient had an access of hectic fever. Purulent matter escaped from the rectum, and was sometimes voided in great abundance. This gradually diminished, and ceased altogether by the 10th. The woman then left home for three weeks, during which period, and until the 15th of December, when a menstrual secretion took place, she experienced tightness in the belly, with occasional pain. From the first appearance of the menstrual discharge she enjoyed good health.

Mess. Dubois and Désormeaux were appointed to investigate the merits of the operation; and their report is so far favourable as to recommend its adoption in those unfortunate cases where the surgeon may be called too late for the successful employment of milder means. The fact of pregnancy in this case may be fairly questioned if not denied, and from some circumstances it may almost be supposed that the uterus was not implicated in the disease; but the difficulty is thus overcome by Mess. les Commissaires. "It is seen that in the period of pregnancy (at least the end of the fourth month) when the operation was performed, the remains of the fœtus (and placenta) *may be dissolved*, and disappear in the fluids furnished by the membranes without our being able to recognize them in any distinct form." The case and report are both noticed in a paper written by M. Montegre, * who observes, that "puncture of the uterus being always an important and dangerous operation, two other modes of relieving the case have been proposed;—division of the symphysis of the pubis by M. Gardien, and the more eligible mode by Capuron, who, because in retroversion the long diameter of the uterus answers to the short diameter of the pelvis, proposes, before any attempt is made to replace the tumour, that the operator shall force the fundus of the womb towards the right sacro-iliac junction, so that its neck may be turned towards the opposite side, in which the rectum descends, an arrangement whereby the gut will be subjected to the least pressure, thus making the uterus correspond with the longest diameter of the pelvis.

With regard to the danger of the operation by puncture, I have already said enough to prove that it has been greatly overrated. With how much saving of distress to the patient it can be practised in some instances, may be inferred from a comparison with some of the expedients heretofore adopted. Among others, for instance, both hands at the same time have been passed into the pelvis. An operation of this kind was performed upon a woman four months gone with child, whose case is published by Mr Weir. "After much difficulty, and a great degree of force, and in opposition to the strong and powerful exertions of the patient, which all tended to prevent its admission, I succeeded in getting my hand into the vagina, forced up my finger above the pubis, and reached the mouth of the womb. An assistant, *at the same time*, got his hand into the rectum, and *we had thus* a perfect command of the patient. Much assistance no doubt was derived from there being two persons employed in the operation." * The woman eventually recovered. Now, the reader (if he has ever had occasion to introduce his whole hand into the rectum) has here to reflect, only for one moment, upon the steps of this proceeding, in order that he may form a correct judgment of the severity and dangerous tendency of such an operation. Mr Weir, in adverting to the use of a trocar, does not know that it has ever been employed, and thinks it never will; indeed, he considers almost any thing more warrantable than puncturing the fundus of the uterus.

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* Dict. des Sciences Médicales, Tom. ix. p. 80. Article Déviation.

Danger and sometimes death will result from *partial* retroversion of the womb; a curvature of the organ whereby the fundus is thrown back upon its cervix, constituting what is not improperly called a *retroflection*. Dr Denman speaks of it as an event which happens only after parturition, from bearing down of the woman before the uterus has been shortened by contraction of its fibres. A distortion of this kind may befal the pregnant uterus, and create much difficulty and embarrassment in practice, as is shown in the following example.

A woman, æt. 40, became a patient of the Birmingham Dispensary in 1822, three days before her death. Although the mother of several children, she was not conscious of pregnancy. Retention of urine occurred ten days previously, during which time an apothecary had charge of the case. He once succeeded, after causing much pain, in obtaining a large quantity of bloody urine, but could never pass the catheter afterwards. When recommended to the dispensary, she was visited by Dr Lee, who requested the opinion of a surgeon. From the circumstance of the woman, whose experience was allowed to have some weight, asserting that she was not pregnant, the diagnosis of the case was obscured. Her belly was generally distended, but not so much so as to prevent the detection of a tumour between the umbilicus and the pubis, inclining rather to the right side. The vaginal passage was so limited by the presence of a tumour in the pelvis, that the introduction of the finger was difficult, even after the bladder had been emptied. The surgeon succeeded once only in introducing the catheter, notwithstanding many attempts were made to carry it into the bladder, and yet the instrument always passed freely, though no discharge of urine followed. Every unsuccessful operation was attended by great pain, and generally by a free discharge of blood. The case was suspected to be one of malposition of the uterus, but all endeavours to relieve the patient proved fruitless, and after much suffering, she died in a state of exhaustion upon the thirteenth day of her illness.

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* Glasgow Med. Journ. Vol. i. p. 268

For an opportunity of being present at the dissection, I am indebted to the kindness of one of my colleagues. After death the belly was still large and tense, and the presence of a somewhat defined tumour still manifest. Before the body was opened, a long catheter was many times introduced, and this with extreme facility, but not in any instance with the purposed effect. The intestines were found pushed up, and the inferior portion of the abdomen occupied by a double tumour unequally divided upon its surface by a tense cord of peritonæum. This division proved to be the natural distinction of the bladder and the uterus. The peritonæum did not exhibit any appearances of inflammation, and the whole of the abdominal viscera were healthy. The boundaries of the uterus could not be traced in this stage of the examination, for the tumour so completely filled the pelvis as to prevent the passage of the hand posteriorly. Fluctuation was evident in both portions of the tumour; but whether caused by a fluid common to both, or held separately in each, did not at first appear. An elastic catheter introduced *per urethram* was discovered in the posterior swelling, the fluid contents of which did not, however, escape. After numerous trials, the instrument at length, it may be said by accident, entered the bladder, and gave exit to more than three pints of urine deeply stained with blood. The anterior swelling immediately subsided, and the uterine tumour became more exposed. The mucous surface of the bladder showed the appearance of high inflammation. The instrument having been withdrawn, was again introduced, but in place of entering the bladder as before, it passed into the uterus, which was now carefully opened, and the catheter found betwixt the membranes and the parietes of the latter organ. Nearly two pints of *liquor amnii* escaped upon a division of the membranes, and a five months' fœtus was in part exposed. The head and chest and upper extremities of the child were still *very tightly* retained in the pelvis. The *os uteri* was placed immediately under the arch of the pubis bearing upon the urethra; the *cervix uteri* much curved, and a considerable part of the organ situated above the brim of the pelvis, its fundus rested in the hollow of the sacrum, thus constituting a case of incomplete retroversion. The catheter had been forced out of the urethra, where it formed an angle, then passing forward in a direct line, entered the mouth of the womb. Effusion of blood in the cellular substance rendered a minute examination of this part difficult, if not impossible, but no opening could be detected in the parietes of the uterus.

It is singular, that in the numerous journies of the instrument along this artificial passage, the membranes of the ovum should always have escaped unhurt. Had they been ruptured, the case might have terminated favourably. Death unquestionably resulted from laceration of the urinary canal, in consequence of which the bladder could never afterwards be properly relieved. This at the time appeared to me a case in which perforation of the uterus might have been practised with advantage; and it was a recollection of this case which led me to perform that operation upon Martin.

ART. III.—*Report of a Singular Case of Malformed Heart.* By EDWARD BLACKMORE, M. D. Physician to the Plymouth Public Dispensary, and Member of the Plymouth Institution.

HANNAH KINGDON, aged 3½ years, was presented to the writer on November 10. It was stated that she was born a fine baby, and no ailment was remarked until she was two months old, when her skin appeared discoloured, and peeled off as if after a fever. From this period she remained *blue* and drooping, with a bad appetite and irregular bowels; at one time requiring astringents, at another purgatives. She suffered in dentition, but never had fits or vomiting. The full number of teeth were formed, but they all subsequently decayed except two. Micturition was habitually difficult; the extremities were alternately hot and cold, but always easily chilled, and of a deepening livid colour. She never walked alone, nor talked intelligibly. Her respiration was always quick, and at times so obscure as to be scarcely sensible. The chest appeared to the mother like that of a person always hurried and beating. Her diet consisted of bread and butter; but she never eat heartily. All her complaints were aggravated since her inoculation at the age of sixteen months. For the last three months she had violent cough; and the bowels discharged green matter for a fortnight past.

She is now extremely puny, indeed more so than many infants only eight months old; yet not with the aspect of ordinary emaciation. The hands are turgid and of a dark blue colour; the face tumid and black, particularly the lips, like that of a person hanged; the gums are decaying; the tongue aphthous.

She moans grievously. The chest is chicken-breasted; the arteries at the inferior part of the trachea are seen to pulsate violently; the heart's pulse is very vibratory to the hand; its rythm regular; the pulse at the wrist is small, very feeble, but regular. Her whole aspect indicated imperfect arterialization of the blood: It was impossible to mistake the *morbus cæruleus.*

Some magnesia with rhubarb only was prescribed. On the 20th, after a flow of black blood from the mouth and nose, death speedily ensued.

Inspection twenty-four hours P. M.—External aspect. Extreme atrophy, particularly of the inferior limbs; a diffused livid discoloration of the trunk; lips black; mouth cankrous.

Internal:—Chest; *First* aspect. The heart so large as to hide and compress the lungs; the right auricle and ventricle, as it seemed, alone visible; and these were half as big again as the entire heart in a subject of this age; the auricle and superior *vena cava* were greatly distended with black blood; the ventricle evidently vastly thickened and firm: ℥4 of serum within the pleuræ; no pleuritic adhesions; ℥i in the pericardium. *Second* aspect on dissection. Lungs small, particularly the left; dense like flesh, even more so than the liver; of a deeper mahogany colour than natural, with no cadaverous lividity, as is common in the posterior parts; no tubercles, nor confirmed induration. The air tubes were filled with much frothy mucus; and the mucous coat of the large bronchi was like scarlet cloth. The left ventricle was extremely small in comparison with the right, but as big as is natural at the present age in health; where the left auricle should have been, only small muscular vestiges were found without cavity.

After a nice dissection of the pericardium from the heart, on attempting to lay open what appeared to be the tip of the left auricle, the knife immediately entered a large single cavity, betwixt which and the left ventricle no communication existed, except through a small fissure; the natural ventricular opening being occluded by a firm adherent fleshy membrane. From the posterior superior part of the right ventricle arose the aorta much enlarged. Its coats were of a dark reddish hue, but the valves perfect. Immediately below the aorta a small pulmonary artery had its origin. The left ventricle held a small coagulum of black blood, which must have entered it by the fissure above described, and which was its sole outlet. Its substance was healthy. No *ductus arteriosus* remained. The valve betwixt the right ventricle and the single auricle was not quite perfect. On compressing the heart the aorta was instantly filled with coagulated blood, which must also have passed into the superior *vena cava* had not a fine arachnoid membrane occluded its mouth. The right auricle proper received as usual the trunk of the superior cava in the site of the *foramen ovale*, and around its orifice was attached the thin delicate membrane already mentioned. The two small pulmonary veins entered the inferior part of the left auricle, which was nearly obliterated, and one large vein entered its superior part. The *sinus venosus* of this auricle was about the size of a small bean. The auricular septum presented the *fossa ovalis* in its natural state, and without communication with the right auricle. It has, however, a direct communication with the left ventricle by means of a small fissure, through which the blood returning from the lungs must have passed, and thence into the right side of the heart. The ribs just where the cartilaginous portion begins had each a large osseous tubercle, which pointed inwards and indented the lungs.

Abdomen.—The liver was very large, but natural in its texture. From its external aspect it seemed as if the right lobe had been imbued with a darker blood than the other parts. The gall-bladder contained bile of an ochrey yellow colour; the spleen was very large, and full of dark fluid blood; the intestines externally exhibited a diffused sublivid vascularity; internally the *valvulæ conniventes* of the ileum were of a dark red hue, vascular, elevated, and covered with what at first appeared to be chyle; but as similar matter was found in the colon it must have been mucus, although very unlike healthy mucus. The stomach was healthy, but small. The mesentery was vascular and livid, and its glands large and darkish.

The heart is preserved in the museum of Mr John Fuge, surgeon at Plymouth.

The above case is highly valuable, as the malformation is so extreme, and in some points unexampled by any cases within the scope of the author's research; and, more particularly, as its living history is so perfect, and the life of its wretched subject was so singularly prolonged, that it greatly illustrates the physiology of the vital organs in health and disease. In most other cases of malformed heart, the previous history is imperfect, as Dr Baillie remarks, and the continuance of life very brief, or the malformation more trivial.

The tumid face, the hemorrhage from the mouth, with the magnitude and colour of the liver and spleen, manifested that some constant obstacle existed to the free transmission of the blood through the lungs, or the right side of the heart; notwithstanding, as the dissection proved, the aorta freely admitted it from the ventricle. The proximate cause of death in Kingdon is certainly to be referred to the progressive carnification of the lungs, from which, as well as from the contracted size of the pulmonary artery, a stop was eventually put to the decarbonization of the vital fluid. May not the predisposition to this morbid change in the lungs have been derived from the preternatural circulation of blood, already more or less oxygenated in the branches of the pulmonary artery? It is not impossible that in a warm climate, or at a more genial season than this severe incipient winter, the life of an individual with such a malformation as that now described may be prolonged.

Her dwarfish form, the undeveloped mind, the impotence for muscular exertion, with the deciduous teeth, and the frigid skin, impressively illustrate the noxious influence of semi-decarbonized or oxygenated blood on the functions of the body. The marvel here is, how existence was so far prolonged under such unnatural circumstances? It is important to reflect in what were the conditions of the circulation different in the present instance from the natural state. The blood returning at the same instant from the lungs and the general body was commingled in the grand auricle, the contraction of which carried the greater portion into the right ventricle, and probably a little into the left ventricle. When these acted the blood must have passed into the lungs, the aorta, and the single auricle again, where it would meet the waves returning from the *venæ cavæ* and the pulmonary veins; but the concussion of this reflux current would be mitigated, more particularly at the mouth of the superior cava, by the fine valvular membrane described above. A more perfect or more turbulent commixture, however, of the carbonated and oxygenated blood in the heart could not have been effected than in the present instance. And as, from the greater than natural disparity in the size of the aorta and pulmonary artery, the quantity of blood returning from the aortic system must have borne a larger than ordinary proportion to that from the lungs, itself also sub-oxygenated, the entire mass of the circulating blood in the arterial system, must have been inestimably and increasingly more impure than in the natural state. Indeed there was here, most invariably, even in the cærulean, a double source of the impurity of the blood in the aortic system. The grand anterior auricle must have held all the blood from the general and pulmonic venous systems; the right ventricle must have also received and projected this double measure, hence its enlarged capacity and vigour. The enormous dilatation of the aorta could not but ensue, more especially as the pulmonary artery admitted less than the usual quantity. In many points, and in respect of the mechanism of the circulation in all points, Kingdon's heart resembled that of a fish combined with that of a snail. The same ventricle was both systematic and pulmonic. " Frogs, lizards, and serpents, have a simple heart, *i. e.* a single auricle and ventricle. In the tortoise and turtle the two ventricles are united by an intermediate opening; but the turtle has two separate auricles, and the large arteries originate in the right ventricle, no vessel coming from the left. The greater circulation is so independent of the pulmonic that it could go on when the animal is prevented from respiring; the cessation of respiration, however, would impede the pulmonic circulation. Whereas in perfect warm-blooded animals no blood can enter the aorta which has not first passed through the lungs into the left ventricle; hence an obstruction of respiration instantly influences the greater or aortic circulation."—Blumenbach.

This case is so important, that, for its fuller illustration, I shall collate it with some other cases of malformed heart on record.

Case I.—Morgagni reports the death of a girl aged 16 years, who had been confined to bed nearly from infancy with laborious and confined respiration, and a livid skin. Her right ventricle was enlarged; the right auricle double the size of the left; the foramen ovale open; the valves of the pulmonary artery ossified and contracted. He is disposed to think that the small size of the pulmonary artery was the cause of the dilatation of the right heart, and of the open foramen ovale, and that some obstacle to the pulmonary circulation is the chief origin of the malformation which occasions the cærulean malady.

Case II.—Dr Baillie has reported a case in Dr Hunter's Museum, where the right ventricle was small in its cavity, but like solid flesh; the pulmonary artery impervious; the foramen ovale open and large. The symptoms were, skin dark; respiration laborious; and the heart's action violent. The subject lived only thirteen days.

Case III.—In another case by Dr Hunter, still-born at 6 months, the pulmonary artery was small; the *septum cordis* had a hole as big as a thumb.

Case IV.—In Dr Pulteney's case, in the Medical Transactions, inability for exertion; deepening tinge of skin; breathlessness and faintness on any effort; death with dysentery, æt. 13 years and 9 months.—*Inspection.* Liver, spleen, kidneys, stomach, and intestines, healthy, except near the end of the ileum, which was vascular. The lungs small and flaccid; left auricle very small; an aperture in the *septum ventriculo-*

sum so as to admit the finger from the aorta into either ventricle; mouth of pulmonary artery contracted.

Case V.—Dr Wollaston's case, æt. 2 months, the aorta arose from the right ventricle and the pulmonary artery from the left; a small *ductus arteriosus* formed a communication betwixt these; *foramen ovale* somewhat open; living history imperfect; skin livid and cold; respiration natural.

Case VI.—Standert's case, by Dr Combe, Philosophical Transactions, 1805, died æt. 10 days; functions regular; skin purplish; body fleshy; an external aspect of the thoracic organs; one auricle observed, into which the pulmonary veins and *vena cava* entered; pulmonary artery awanting. Indeed one auricle and ventricle, which gave off the aorta; the auricle single, a mascular band instead of a septum. From the aorta a small *ductus arteriosus* supplied the lungs. Here an organization like that of amphibious animals.

Case VII.—Spry's case, æt. 17, (Memoirs Medical Society, London,) "existed only." Extremities livid, a wounded part poured out dark blood; heart large and firm, distended by black blood; *foramen ovale* pervious, and very large; so the *ductus arteriosus;* lungs sound; liver very large; texture firm; colour natural. She never menstruated; colour of body blue, deepening with her years; acute and throbbing pain at head and left side, with palpitations; difficult breathing, and occasional cough; fainting on exertion; voracious appetite.

Case VIII.—In Professor Sandifort's blue boy, dyspnœa; pulsations in the neck; tumid livid face; red urine and costiveness; constant shiverings; occasional epistaxis, which relieved him; foul tongue, and fetid breath. (Age not stated.) *Inspection.*—Lungs compressed by a large heart full of blood; some serum in pericardium; right ventricle and auricle vastly enlarged and distended; pulmonic veins turgid; aorta enlarged; pulmonary artery contracted; no *ductus arteriosus;* lungs small, not diseased; aperture in *fossa ovalis,* which would admit a probe; aorta arose from both ventricles; (from an aperture in the septum cordis;) an excrescence on the valves of the pulmonary artery had so far closed its lumen as to admit only a small probe.

Case IX.—A species of malformation described by Wilson. Heart of one auricle and ventricle, from which a large artery arose and was divided into two branches, one to the lungs, the other distributed on the aorta; the great pulmonic veins joined the superior cava and formed the grand auricle.

Art. IV.—*On the Cause of the Milky and Whey-like appearances sometimes observed in the Blood.* By Robert Christison, M. D., Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c.

In our present state of ignorance as to the chemical pathology of the human fluids, every addition to our knowledge, more especially of the pathological states of the blood, must be a subject of interest, although its precise bearings on physiology and medical practice may not at first be apparent. Within these few years several interesting additions have been made to this department of medical knowledge, which have admitted of very direct and important practical applications. No one who is acquainted with the researches of Drs Marcet, Prout, Bright, and Bostock, will be inclined to doubt the practical utility of pathological chemistry. On the contrary, he will rather regret that so few physicians carry with them from the schools the knowledge of the details of organic chemistry, which is requisite for carrying on this kind of investigation.

It is not easy to say what are the relations which the addition to be made in the present paper to the pathological history of the blood may have to physiology or the practice of medicine. The state of the serum which I propose to investigate is, in its minor degrees, not uncommon. But the circumstances in which it occurs have not hitherto been often enough observed to justify any pathological or physiological inferences. The facts, however, appear to me too interesting not to deserve being recorded. I must therefore be content with simply making them public, and directing the physician's notice to an easy method of chemical investigation in all cases of the kind.

In the last number of the Edinburgh Medical and Surgical Journal, some observations have been made for the purpose of illustrating a case communicated by a correspondent, where the serum of human blood presented, when recently drawn, the appearance of milk. The author of the observations appended to the case has quoted several parallel instances from the experience of *Hewson*, and extracted from the same writer copious references to other sources of information in earlier works. From these particulars, from the more accurate researches of later experimentalists, which will presently be mentioned, and likewise from the facts which have come under my own notice, or been communicated to me by my friends,—it would appear that a milky-like state of the serum of human blood is not a very uncommon occurrence.

Various opinions have been formed as to its cause. The most recent conjecture is that of the correspondent whose case appears in the last number of the Medical and Surgical Journal, and who takes it for granted that the milky-like serum is nothing else but milk. *Mr Hewson*, after stating that he was at first led to imagine the milkiness depended on a portion of the chyle not being converted into blood, acknowledges that this idea was not borne out by facts. He afterwards observed, that on, evaporating milky serum to dryness, the residuum made a greasy stain on paper; from which he infers that the cause of the milkiness is oily matter diffused throughout the blood; and he considers that this oil is in all probability formed by the mouths of the absorbents from the fat of the adipose cells.* The late *Dr Gregory* observes explicitly, but without specifying any grounds for the statement, that the milkiness depends on the absorption of fat and its non-conversion into gluten or fibrin.†—The only person, however, who has hitherto carefully analyzed milky serum, and been thus enabled to assign a substantial reason for the opinion he espouses, is *Dr Traill* of Liverpool. His researches may be seen in various numbers of the Medical and Surgical Journal for the years 1821 and 1823. He analyzed with care three specimens of milky serum, two of which were taken from the same person in two different illnesses. The specific gravity was under the healthy standard in all, being 1018.7 in one case, and 1025 and 1018 in the two specimens from the other. The former serum contained 21.1 parts per cent. of solid matter, a most unusually high proportion; and of this 15.7 were albumen, 4.5 an oily matter, 0.9 saline substances. Of the two specimens in the latter case the lighter gave 16.4 per cent. of solid matter, 13.3 being albumen, 2.5 oil, 0.6 saline substances; while the denser specimen gave 15.2 of solid matter, of which a considerable proportion was oil. From these analyses *Mr Traill* concludes, that oil exists in all kinds of milky serum, and that the milkiness is partly perhaps owing to this oil, but chiefly in all probability to an excess of albumen. ‡ Since his researches, notice has again been taken of the occurrence of milky serum in two numbers of the London Medical Repository for 1824. One of the editors of that Journal says he has four times witnessed a milky or whey-coloured appearance in serum; and (apparently under the impression that he was the first to notice it, and attempt to explain it,) puts the question, whether the appearance arises from the presence of oil. * In a subsequent Number, *Mr Venables*, a correspondent of the Journal, also mentions his having seen the appearance in question, but denies that it depends on the presence of oil, though without assigning any reason.†

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* On the Blood, 141, 148, 149.

† Conspectus Medicinæ Theoreticæ.

‡ Edin. Med. and Surg. Journal, xvii. 236. *Ibid*, 637, and xix. 321.

My own experiments, which will now be detailed, have furnished results in many respects the same with those of *Dr Traill*. They differ, however, in some subordinate particulars, and likewise in leading to the conclusion (which I trust the sequel will satisfactorily establish,) that the cause of the milkiness is simply the fat of the human body contained in the blood in the form of emulsion.

I am indebted for the milky serum which I examined to *Mr Ziegler*, a surgeon of this city, who had the kindness to submit to my inspection two specimens taken from the same patient, with an interval of a week between them. The blood which yielded them had a whitish appearance before it coagulated, so that *Mr Ziegler* at first imagined milk had been accidentally left in the cup. The blood each time coagulated in the customary way. In appearance both specimens of serum were quite undistinguishable from new milk, being at least as white and opaque, especially the serum of the first-drawn blood.

The specific gravity of both was much under the healthy standard, that from the first drawn blood being 1018.7, that of the last drawn 1021.8; while that of a specimen of healthy serum examined at the same time was 1030.8, which is rather above than below the average. The quantity of solid matter was above the healthy standard in the lighter milky serum, and about the healthy standard in the heavier. The solid contents of the healthy serum dried at 212° till they ceased to lose weight amounted to 10.22 per cent., which is about the average assigned by chemists; that of the lighter milky serum was 13.77; and that of the denser serum 9.81. It is worthy of remark, that, neither in relation to healthy serum, nor to one another, was there in these specimens of milky serum any direct proportion between the density and the solid contents; the proportion was rather an inverse ratio. The same fact results from the experiments of *Dr Traill.*

The solid contents of both varieties of milky serum were obviously greasy on the surface, so as to stain glass and impart an oleaginous appearance to filtering paper. This was also remarked by both *Mr Hewson* and *Dr Traill.* The oleaginous or rather adipose matter, however, may easily be separated in a state of purity from the original serum by means of sulphuric ether. As ether does not coagulate serum, and possesses the power of dissolving most oily and fatty substances, I expected, by agitating the milky serum with a larger quantity of ether than the water of the serum can dissolve, that the adipose substance would after repose rise to the top with the undissolved ether in the form of a pure ethereal solution. Accordingly, when the milky serum was agitated with half its volume of ether in a small, narrow, precipitating glass, the milkiness in a few seconds disappeared entirely; and a nearly transparent fluid was formed, which, when left at rest for a minute or two, divided into three layers. The uppermost was colourless and transparent;—the second greenish, translucent, opaline, and apparently formed of little irregular masses, not unlike the granules of orange pulp;—and the lowest greenish and nearly transparent. The upper layer was a pure ethereal solution of adipose matter; which, as the ether evaporated away in a watch-glass, collected in oily-like globules on the glass. The second was a mixture of much ether, a good deal of albumen, and a little adipose matter. For, when left exposed to the air, the adipose matter, as the ether evaporated, gradually collected on the margin of the mass; and the remainder of the residuum, composing its greater part, was evidently albumen with its original properties nearly entire, especially its coagulability under the application of heat. The lowest layer was pure serum containing a little ether in solution, but quite free from any greasy impregnation. When exposed to the air till the ether evaporated, a faintly-gelatinous mass was left, which presented all the chief properties of uncoagulated albumen.

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* Lond. Med. Repos. xxv. 191. † *Ibidem*, xxv. 257.

The effect of agitation with ether in the preceding experiment shows, that the milky state of the serum is owing solely to some oily or fatty substance in a state of intimate suspension or emulsion; for the milkiness is at once and entirely removed by a fluid which possesses the property of dissolving the fatty matter. That an excess of albumen has nothing to do with the phenomenon, as *Dr Traill* conceived it had, is evident from the fact, that the milky serum of the second-drawn blood did not contain more solid matter than usual. Besides, it is well known, that white of egg, which contains much more albumen than was present in any of *Dr Traill's* specimens of serum, is colourless and transparent when uncoagulated; and it is not easy to conceive how an excess of albumen should impart whiteness and opacity to one fluid, and not to the other.

I have now to consider what is the exact nature of the fatty or oily matter thus separated from the blood. In the first drawn specimen of serum, the fatty matter formed about 5 per cent of the whole; and in the second specimen 3 per cent.; that is 36 and 32 per cent. of the whole dry residuum procured by evaporating the serum at 212°. In the former it was translucent and nearly colourless, when solid; in the latter it had a yellowish tint. Both had a faint, peculiar, heavy, oleaginous odour. Both were concrete at 60° F.; at 70° they began to soften, but remained opaque; at higher temperatures their fluidity increased, but they were not sufficiently fluid to flow easily till about the temperature of 96°. Both were inflammable, and burnt entirely away with a clear white flame. This property is best ascertained, as *Dr Traill* suggests, by kindling the substance under trial on a small parcel of asbestus fibres. Both contained two adipose principles,—one fluid at ordinary temperatures, the other concrete; and these principles were easily separated by cooling the compound substance down to 40°, and placing it between folds of filtering paper. The paper became impregnated with an oil, fluid at 40°; while a solid fat was left behind, which was concrete at 100° F., and liquid at 115°. The former was therefore obviously the *oleïne*, and the latter the *stearine* of *Chevreul*. The quantity of the compound matter at my disposal was too small to allow me to ascertain whether any other fatty principle was also present. But the properties I have enumerated are sufficient to show that the composition of the fatty matter procured from the two specimens of serum corresponds in every essential respect with the account given by *Chevreul* * of the composition of human fat, which consists of stearine and oleïne alone in various proportions, and one specimen of which, taken from the neighbourhood of the kidneys, he found to have nearly the same point of liquefaction as the compound now under consideration.

It hence appears that the opinion of *Dr Gregory* regarding the cause of the milkiness of serum is correct, and that it depends on the absorption of fat.

Hitherto I have considered only the milky state of the serum, or that state in which the serum does not differ in appearance from milk. But there is another condition of much more frequent occurrence, and which is often, though incorrectly called milky, where the serum has the appearance of whey,—being translucent, and grayish by reflected, but yellowish by transmitted light. This appearance is not unfrequently seen in cases of dropsy with coagulable urine, and also in acute rheumatism. So far as I know, its nature has never been particularly examined.

I have analyzed four specimens of this whey-like serum, all taken from persons labouring under dropsy with coagulable urine, and whose blood formed in every instance a buffy coat on coagulating. My attention was chiefly turned to the possibility of discovering fatty matter in them; and in each I was successful. By means of pure sulphuric ether, used as formerly related, I separated a fatty matter, exactly agreeing in properties with that procured from the milky serum of *Mr Ziegler's* patient. One of these specimens of serum, which was put into my hands the other day by *Dr Alison,* appears to contain fully one per cent. of fat.

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* Recherches sur les Corps Gras d'origine Animale. 1823. P. 249.

I may farther add, that the serum, even when not whey-like, but perfectly transparent, occasionally contains a trace of fatty matter. I have examined some specimens of serum, where I could not detect any fat; but in two instances, where that fluid did not appear to differ from the healthy state, a distinct trace of the same fatty impregnation was discovered. It is hence extremely probable, that in the natural state of the blood there is generally, if not always, contained in it a greater or less proportion of a fatty matter, exactly corresponding with the fat of the adipose cells. If this be afterwards proved to be the case it will be an additional circumstance to be placed along with the facts already discovered, which show that various principles found throughout the body and in its secretions, exist in the blood, and are therefore secreted from it simply by a process of elective percolation.

It would be premature to attempt to connect the existence of a considerable proportion of fat in the blood with other more palpable pathological phenomena, as a sufficient number of facts have not hitherto been collected on the subject. Meanwhile, however, it may be stated, that it appears to be connected in general with an excited state of the circulation, or with that state of the blood in which the buffy coat is formed.

In *Mr Ziegler's* case the patient was bled on account of muscular pains in the left side, accompanied with fever; but the blood was not buffy. In two of *Dr Traill's* cases the disease was hepatitis, which in one was acute. In his third case it was nephritis. The case mentioned in the last number of the Medical and Surgical Journal was an acute inflammation of a doubtful nature, probably pleurodyne or pericarditis. Of *Mr Hewson's* four cases one was acute rheumatism, another active epistaxis, a third slight asthma, and the fourth an anomalous case of suppressed menses, with pain in the left side and a frequent pulse. The whey-like state of the serum, which I have often seen before my attention was particularly turned to its cause, has, so far as I remember, occurred to me most distinctly in cases of acute inflammation, or in cases of dropsy with coagulable urine, where there is a strong inflammatory tendency in the body and very often buffiness of the blood.

As the milky appearance has occurred chiefly in persons abruptly seized with an acute disease, it occurred to me that it might simply arise from the absorbents acting powerfully on the fat of the body on the person being suddenly restricted to low diet. *Mr Ziegler's* case, I conceived at one time, favoured this notion. She was a corpulent female, twenty-seven years of age, and had been for some days on low diet before she lost blood. I am informed, however, that she has been since bled more than once for acute complaints, without any such appearance being witnessed in the blood as that observed on the former occasion.

The most convenient method of searching for fatty matter in the serum, is to agitate in a long narrow phial, half an ounce, or a whole ounce of the serum, with about a third of its volume of ether for one minute; and to leave the mixture at rest for a few minutes or half an hour, according to the rapidity with which it divides itself. The uppermost clear stratum is then decanted off or removed with a pipette, and evaporated in a little glass vessel or other cup at a gentle heat, such as before a common fire.

Art. V.—*Memoir on the Developement of the Organs of Respiration in Birds and Mammiferous Animals.* By Professor Rathke. Published by G. Breschet; abridged and translated from the Répertoire Génerale d'Anatomie et de Physiologie Pathologiques, Tome vii. Part I.

THE subject of the present memoir, though not much attended to in this country, is one which, for several years past, has excited much interest and inquiry on the Continent. In Germany especially it has been successively studied by Wolff, Oken, Meckel, and Tiedemann, and more recently by Baer, Huschké, and Rathké. The results obtained by the three latter inquirers have been so far satisfactory that the knowledge of the embryal developement of the different organs may be now said to assume the shape of ascertained facts; and Charles Frederic Burdach, an eminent German author, has attempted to reduce them to the shape of a generalized system.* Though this work, therefore, contains the general results of the inquiry, and exhibits the successive stages through which it has passed, and may therefore come under notice on a future occasion, we believe that the subject is too important to be presented in that form only; and we doubt not that our anatomical readers will be pleased with seeing a short view of the researches of Professor

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* Die Physiologie als Erfahrungs wissenschaft Zwey Banden. Bearbeitet Von Karl Friedrich Burdach. Mit Beitragen, von Karl Ernst von Baer, Heinrich *Rathké* und Ernst H. F. Meyer. Leipzig, 1828.

Rathké, as published by M. Breschet. These researches, as well as those of Huschké, made their first appearance in the Isis of Oken,—a periodical work which we have before had occasion to mention as the most valuable in Germany, for the original character of its materials. It probably ought to have been preceded by those of Baer and Huschké, or even a general view of the facts ascertained by Wolff, Oken, and Meckel, in order to assign to each his just degree of merit. This, however, is at present unseasonable, and our limits allow us to select only what is most important and most likely to lead to interesting results in the theory of organogenesy. The whole subject exhibits the most interesting train of analogies in the various forms of living bodies, and furnishes the means of explaining not only some curious facts in organization, but many varieties of malformation.

§ 1. When we attentively examine the chick *in ovo* towards the third or fourth day of incubation, not only is the length of the cervical region in relation to the trunk considerable, but its size is remarkable; and when dissected cautiously, and with the aid of good instruments, it is found to enclose a cavity proportionally large, extending from the buccal orifice in the shape of a cone, and tapering insensibly, for a good space beyond the point where the aorta issues from the still single ventricle of the heart, to penetrate the substance of the body. The posterior part of this cavity, which is smallest, is continuous with those of the œsophagus and stomach. As the evolution of the chick proceeds, the anterior part of the cavity and the enclosing parts are gradually shortened in proportion to the trunk, till the space constitutes part of the pharyngeal cavity. At this time, from the third and fourth days, and even to the fifth, it resembles the space with which the branchial cavities of fishes, especially that of the *squali*, communicate. On the sixth and seventh days this resemblance is no longer distinct. In the three lateral walls of the cavity, as in the tribe of fishes, are seen several slits extending from above downwards. In the fowl their number is three; in the shark tribe they are five. The anterior is longest and largest, the posterior smallest. All three penetrate the substance of the wall of the neck, so that the external orifice of each, which is a little longer and broader than the internal orifice, appears at the surface. From this it results, that on the third and fourth days of incubation, there are in the side of the neck of the chick three cavities flattened from before backwards, diminishing in breadth from without inwards, and communicating with the general internal cavity, parallel to the axis of the body. The latter, which is the pharyngeal, as also the six lateral and transverse cavities of the neck, are lined by a very soft and delicate, but already distinct continuous membrane. In the chick on the fourth day, I have often succeeded in detaching it in shreds; and examined both by the microscope and powerful lenses it appeared uniform at all points.

§ 2. From the disposition of the three slits in each lateral portion of the neck, there result two *septa*, partitions, or intermediate plates, one side of which is anterior, the other posterior. Their external margin, which is convex and raised above the level of the neck, is thinner and longer than the internal concave margin. The position, situation, and shape of these plates give them a remote resemblance to gills, and suggest the inquiry, whether at a later period they do not acquire a closer resemblance to these organs, and whether in fishes the gills have not, at first developement, the same aspect as the plates observed in the chick. As to the first circumstance, the observation of the further developement of the chick proves that the plates disappear shortly after this epoch; and, in regard to the second, the figure of a very young embryo of the *Blennius viviparus*, given by Forchhammer,[*] furnishes a presumption that the gills of the early embryos of the osseous fishes have the same position, situation, and shape as the septa of the neck of the chick.

This inference I regarded as established, when I observed the developement of the *Blennius viviparus*. I found that, in the first half of the embryal life of these fishes, when the lower jaw begins to be formed, the *operculum* and branchial membrane are scarcely indicated on each side, and that they appear under the shape of very narrow prolongations of the mandibula, from which they are parted by a superficial furrow visible without, and that they leave quite uncovered the first traces of all the gills. These branchial rudiments appear, however, at the different periods, like rude plates, curved like arches, of moderate relative thickness, narrow, short, directed with little obliquity downwards and forwards, and proceeding directly, not obliquely, outwards. These plates are situate near each other, present hitherto no trace of lamellæ or branchial divisions, and resemble closely the plates of the neck of the chick now described, save only that they are more numerous and proportionally thinner. The gills of the shark and ray families are originally also very similar to those of osseous fishes,—a circumstance which I have endeavoured to render probable, in a memoir inserted in the fourth Fasciculus of my contributions to the History of the Animal Kingdom. Judging by figures given by MM. Prevost and Dumas, it appears that frogs, in the early period of developement, present thick plates of the same shape and position as those of the chick.

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[*] De Blennii vivipari formatione et evolutione. Kiliae, 1819.

From the analogical circumstances now enumerated, I think I am not mistaken in regarding the plates found in the neck of the chick as rudiments, or one of the lowest transitory forms of the gills, and in maintaining that birds during the first periods of embryal existence have gills, which, nevertheless, disappear as the process of evolution advances.

§ 3. The outer surface of the lower side of the cervical region of the chick on the third, and especially the fourth day of incubation, presents the following peculiarities. Immediately behind the buccal orifice are two large thick lobes belonging to the two lateral halves of the chick, and formed of the same gelatinous substance as the rest of the body. These lobes are united below on the median line, but so that their point of junction is denoted by a large superficial furrow; and above they are continuous with the substance of the nape of the neck. The lower surface of each lobe presents a transverse superficial furrow dividing it in two parts. The anterior by gradual developement forms the *lower jaw.* The posterior constitutes with its posterior margin, which is moderately acute, a slight projection downwards, and by its posterior region covers the anterior or largest slit, much in the same manner as the *operculum* covers the gills in most fishes. This hinder part of the lobe further corresponds with the *operculum* of adult fishes, which, according to the researches of Bojanus, could be only part of the lower jaw, in this respect, that its greatest breadth is in the middle, while it contracts from this point upwards and downwards. Behind this the cervical region becomes narrower below; and on this contracted spot, that is, at some distance behind the part which represents the *operculum*, are placed the orifices of the other two slits of the neck. Immediately behind the two anterior lobes of the cervical region begins the upper and smaller portion of the cavity which lodges the heart, and which, under the form of a large but superficial depression, is divided beneath and between the slits, and stretches a little behind the openings. Of this cavity the greater part is formed solely by the pericardium, which is still entirely uncovered, and which is attached above to the slightly prominent margin of the aforesaid depression, which is placed on the lower side of the cervical region.

§ 4. The heart, which on the third and fourth days of incubation consists only of *one* ventricle and *one* auricle, then gives birth to the *aorta*, under the form of a bent tube of moderate length, penetrating the substance of the body, behind the two lobes already mentioned, and between the two posterior slits of the two lateral halves of the neck. The moment it penetrates the cervical region, this vascular trunk divides like a brush into four separate, delicate branches, two of which go to the right and two to the left. Each of these enters one of the plates or *septa* already described as rudiments of gills, proceeds directly from below upwards, parallel to the outer margin of the table, near which it divides and finally unites below the rudiments of the vertebral column with the three other branches of the aorta, in order to form anew a single trunk, which afterwards constitutes the abdominal aorta properly called.

The disposition of the principal arteries of the body now indicated, I have observed in several embryos which I placed in pure water after removing them carefully from the *ova* with the *blastoderma*; and it is so distinct that it is impossible to entertain any doubt of it. Of this fact it is easy to obtain satisfactory evidence; for the aorta of embryos of this period presents no branch so large as the four now mentioned, which ascend across the neck.

Several times have I examined if these branches of the aorta send lateral twigs into the substance of the plates in which they are enclosed, as the branchial arteries of fishes furnish lateral branches; but these divisions I have never found. The trunk was always single.

It is probable that in the first period of evolution in fishes the disposition of the arteries issuing from the aortic arch is equally simple, and that they go to the aorta in the same manner as in the young embryos of the fowl, but that, at a later period, they ramify in a lateral direction in proportion to the developement of the lamellæ. The middle portion of each branch diminishes as the lateral twigs enlarge; and finally, the principal branch appears to divide into two separate vascular trunks, mutually united by lateral branches, and which are distinguished respectively as the branchial artery and the branchial vein. This view, which is only supposition however, derives consistence from the connection between fishes and salamanders. In the latter family of animals the developement of the vascular system is exactly that now stated. According to the beautiful researches of Rusconi, indeed there is in these animals originally one vessel only, equally large every where, which is distributed round each gill; at a later period, in proportion as plates are formed in the gill, the vessel, hitherto single, transmits into each of the *lamellæ* two different branches continuous with each other in the form of an arch; while this process is going on, the vessel itself undergoes a modification so as to present its greatest size at the origin, and its smallest capacity at the termination of the gill; and in this manner it divides into a peculiar artery and vein.

From the foregoing facts, it results that the disposition of the anterior portion of the arterial system in birds is at first the same as that of salamanders in the first stage of their evolution; since at this period the vessels of birds present a striking resemblance to the branchial vessels of salamanders during the first stage of developement. From this disposition of the vascular system, it further results, that the portions which I regard as indications of rudiments of gills in the chick are genuine examples of these organs,—an evident and important proof of the proposition, that the higher classes of vertebrated animals pass in the course of developement through the degrees of organization of the lower classes.

§ 5. On the fourth day of incubation, by dividing the uncovered pericardium and removing the heart, the œsophagus is brought into view in the shape of a short canal adhering by its lower margin to the upper wall of the pericardium. This canal is proportionally large. Its lower wall is provided all along with a large but thin margin, which renders it thicker and more opaque than the upper wall. In some embryos I found the lower wall every where equally thick; and in others it was thinner on the median line than on the sides. This greater thickness of the lower wall depends on the circumstance, that it consists of two layers, the upper of which belongs to the œsophagus, and the lower to the *windpipe*, which it represents at first developement. These two layers are, however, inseparably blended, and it is only after some days that they can be distinguished.

To show that this greater thickness of the lower wall of the œsophagus depends on the commencement of the formation of the windpipe, it is merely requisite to examine attentively the transit of the alimentary canal in the abdominal cavity. Immediately behind the pericardium is remarked the large thin margin of the lower wall, thicker at the beginning of the intestine, dividing into two lateral excrescences, diverging backwards, and the further developement of which shows them to be the *lungs;* behind these excrescences the lower wall is as thin as the upper one; and only at a certain distance behind the first rudiments of the lungs, the lower wall becomes thicker, as the others, to represent the stomach.

The rudiments of the lung consist on the fourth day of small thin lamellæ flattened laterally, contracting behind and terminating in an obtuse point. Each lung, moreover, does not extend beyond the lower margin of the œsophagus or the windpipe at this period. They are directed along the intestinal canal immediately before the stomach, obliquely upwards and backwards; in some embryos they adhere in their whole extent to the intestinal canal; in others they are loosely attached by their superior margin. From these facts, it results that in the chick the windpipe, as well as the lung, are developed from the intestinal canal; and it is even probable that the lung appears somewhat before the windpipe.

§ 6. On the fifth day of incubation the two posterior *branchial slits* on each side are so diminished in certain embryos, that they form minute round holes scarcely visible, and in others they are entirely obliterated. When in the latter the pharyngeal cavity is divided, and its walls are unfolded on each side at the spots previously occupied by the two last branchial slits, there is an equal number of minute depressions, which are the last traces of the branchial cavities, and which are eventually effaced in a few hours.

The large lobe situate on each side behind the buccal orifice is moderately enlarged, and its transverse furrow is deepened. In relation to the posterior portion of this lobe, which has some analogy with the *operculum* of fishes, it is to be observed that its posterior terminal part having become thinner, forms a stronger prominence, either below or behind, than on the previous day, and that it consequently covers more completely the remaining anterior branchial slit, the size of which is a little enlarged.

When in an embryo of five days the pharyngeal cavity is divided above and exposed, we observe above, at the inner surface of the wall forming this cavity behind the rudiments of the lower jaw, two eminences in juxtaposition not well marked, and beyond a third without match. The two first afterwards form the tongue, and the third constitutes its root. A little behind the latter, there is on the fifth day the *glottis* in the form of a short narrow slit, apparently situate on an eminence. Since the glottis is placed behind the slits of the neck, the larynx cannot be formed from the partitions of these orifices, as might be imagined.

§ 7. On the fifth day the *lungs* and *windpipe* are a little more developed. To see the latter, it is sufficient to divide the pericardium still uncovered and remove the heart. But, strictly speaking, this tube consists then only of two *bronchi*. Immediately behind the glottis, beneath the œsophagus, there is a short depressed prominence, tapering behind, which immediately after its origin becomes parted into two branches, which, becoming a little narrower, and adhering to the œsophagus, pass along that canal, upwards and backwards, to the point where it passes behind the heart into the abdominal cavity.

The short anterior portion is the *trachea;* the two larger branches are the *bronchi.*

The tracheal trunk on the fourth and fifth days appears greatly thinner on its median line than at the sides. Hence the changes effected in the trachea within twenty-four hours consist in the lower or œsophageal layer being parted throughout its whole length into two distinct lateral halves by the attenuation of the median portion, and the thickening of the lateral parts. The proportional shortness of the tracheal trunk to the greater length of the bronchi in the first hours of developement, recalls the similar, though permanent, arrangement of the windpipe, in many of the chelonian family.

§ 8. The *lungs* at this period are situate immediately behind the pericardium in the space bounded by the angle formed by this serous sac with the abdominal cavity, or rather with the renal capsules, beside the anterior part of the stomach, which afterwards forms the gizzard. Each lung presents the shape of a small mucous mass flattened laterally, elongated, and with blunt and rounded extremities, not half the length of the stomach. In three embryos of the fifth day inspected expressly, the upper margin of the organ adhered closely to the stomach, but the greater part of its inner side was simply applied, and was easily detached. In other respects, this very delicate part, after induration in alcohol, appeared to be without hollow in two embryos; in a third, it contained a cavity very small in proportion to its thickness.

§ 9. I have above remarked, (§ 6,) that the two posterior branchial slits on each side are generally closed on the fifth day. When this takes place, the part situate on each side before the heart, and which is formed by the union of the branchial *septa*, appears to be shortened both relatively and absolutely, and to disappear altogether in the course of the following day. This total disappearance, however, is only apparent; and at the beginning of the fifth day, when the lateral *parietes* of the abdomen and chest are developed with more energy, the part now mentioned is so intimately blended with the thoracic parietes, that it can no longer be distinguished as a peculiar part.

The *lower jaw* is on the sixth day already more prominent; after that the furrow between it and the *operculiform* process becomes deeper, so that the two regions are separated by a well-marked neck, which, lengthening every hour rapidly, insensibly forms the long neck of the fowl.

What I have now said appears to be at variance with the remark made above, that the larynx is originally formed behind the last pair of gills. Both assertions, however, are true, as I believe I have ascertained by numerous researches. The question to be determined is, how it is possible, though the larynx is found originally behind the operculiform process, for this part to be carried gradually to the back of the neck, without at the same time displacing the larynx? To give a satisfactory answer to this question, it is requisite to consider what becomes afterwards of the operculiform process.

Researches made on this body show that almost the whole of it, at least its posterior and inferior parts, are converted into integuments. In the form of general integument it extends backwards, and is applied over the larynx. After it has passed this organ, it is rapidly prolonged, absolutely and relatively, and with it also is prolonged the part of the neck situate behind the larynx, and which is at first very short.

On each side, behind the operculiform process, which lengthening during the following days, concurs to close below the thoracic cavity still open, at the beginning of the sixth day the branchial slit already described is still seen traversing the wall of the neck. This slit, however, is smaller than on the previous days; and it disappears altogether at the end of the sixth day, during the insensible developement of the lungs. This obliteration is effected either by the lateral parts of the *operculum* being blended with the general integuments behind, or by the parietes of the slit behind the operculum mutually approximating and uniting.

§ 10. On the sixth day the trunk of the *trachea* is a little larger than on the fifth, and the two *bronchi* are proportionally shorter. Both trunk and branches are quite distinct from the œsophagus to which they are loosely connected; and both are still flat, and appear to be without interior canal, even when inspected by the lens. The glottis, in the shape of a short narrow slit, is situate in the middle of a small eminence immediately behind the root of the tongue.

The *lungs*, a little enlarged, continue to represent the bodies something longer than broad, contracting behind and terminating in a rounded apex. They are at the same time carried a little higher along the stomach to the anterior part of which (the gizzard) they are connected by part of their internal margin, and a narrow ligament detached from their upper margin connects them to the suspensory ligament of the stomach, immediately above this viscus. As to structure, the lungs present a more distinct hollow than on the fifth day. But the cavity, probably single, is confined to the lower half of the lung; while the upper half, which is smaller, is absolutely solid, and consists of a soft amorphous jelly. On the limits between the upper and lower halves the substance of the lung is most dense, as may be inferred by the greater opacity and clearer colour of this part, which, like a streak, crosses the lung obliquely backwards and upwards. Observation shows that by developement this streak ultimately becomes the principal bronchial tube, proceeding to the internal side of the lung.

The lower vesicular part of the lungs which has been now mentioned, marks the commencement of the *air-cells* peculiar to birds. The upper, smaller, and solid part is, on the contrary, the space in which are developed during the subsequent days most of the *air-tubes* (bronchia) of the lung. Whether the lower vesicular part, which presents only a wall moderately thick, forms the same cavity with the continuation of the bronchial tube, which traverses the organ obliquely;—in other words, whether this tube is already hollow, and if it is open originally at its lower end as far as it is lodged in the lung; or, if it is still solid at this period, and consequently has no communication with these two parts, my researches have not yet enabled me to determine. It appears to me probable that it is not yet in existence. But, whatever be the fact, I have seen that on the fifth and sixth day of incubation the cavity in the lung belongs to that part of the organ which is afterwards converted into air-sacs, and that there are as yet no air-vessels; that they are developed on the following days; and that, consequently, the cavity exists before the air-vessels. It may therefore be maintained, that at a certain period of evolution, the lung of birds is analogous to that of reptiles, especially to that of several tailed batrachians, and that it presents nothing but a similar inferior lung; and that afterwards there is united to this inferior form of organization a superior one, and that then these two forms are developed separately, unite intimately, and continue during the life of the bird.

§ 11. After describing the formation of the lungs and windpipe, I shall endeavour, before proceeding to the final developement of these organs, to explain the manner in which *the connection between the arterial system and the lungs is insensibly established.*

I stated, (§ 4,) that during the first days of incubation, the heart transmits four vessels which traverse the four branchial septa, and which are again united under the rudiment of the spine. These vessels are very distinct on the third day of incubation; but afterwards, when the *septa* are blended either mutually or with contiguous parts, one of the two vessels mentioned, which are situate on the left side of the body, disappears; and the heart transmits then only three vessels, which unite and form the *abdominal aorta.* Malpighi, Pander, and other naturalists, who have studied the developement of the chick *in ovo*, saw and represented this last vascular disposition; but they failed to recognize its relation and true import.

The assertion of Malpighi, that he saw this vascular structure about the fortieth hour, is doubtless erroneous, and is to be explained in this sense, that he presumed that this structure, afterwards distinct, had the same form some time before. Originally, the vascular trunks now described are situate within the substance of the branchial *septa*. But when the neck is elongated during the subsequent days, and the thorax is closed, the three remaining trunks come to be placed quite freely in the beginning of the thoracic cavity,—a change which is to be ascribed to the influence of absorption taking place in the internal parts of the *septa*, and which has also the effect of elongating the larynx from the chest.

By whatever mechanism this change is effected, the three trunks situate behind the neck are found on the sixth day in the commencement of the thorax. Two of these are then immediately anterior to the gizzard, on the right of the œsophagus; the third is on the left of this canal; and below the spine they coalesce and form the abdominal aorta. When we trace the further passage of these three trunks, the two external are found, one to belong to the right, the other to the left half of the body, establishing the connection between the lungs and the arterial system. Immediately beneath these two external trunks, which afterwards unite and issue from the right ventricle, while the middle alone issues as the aorta from the left, the lungs are then evolved, each soon receiving a considerable twig from the branch of the corresponding side. This arterial branch is then dilated as the lung is developed, and it conveys to the organ a quantity of blood always on the increase. But since this blood is brought back by appropriate veins developed during this interval, and establishing a communication between the lung and the left auricle, which it does not enter, consequently, in the posterior branch of this tube, a disproportion is insensibly established in the size of the two parts of the aortic branch, the one before, the other behind the arterial tube which goes to the lung,—or, in other words, the posterior part is contracted in relation to the anterior. When at length the chick issues from the egg, blood no longer enters this posterior portion; the whole goes by the anterior part of this branch, which has now become the *pulmonary artery*, and daily enlarges as it passes through the lung.

§ 12. On the seventh day the trunk of the trachea is longer, and the two branches are relatively shorter than on the sixth day, —a change which is not only relative but absolute. On the seventh day, however, this absolute shortening attains its maximum; for afterwards the bronchi are elongated anew absolutely, but become insensibly shorter relatively to the trunk of the trachea, the length of which is much increased. At the bifurcation is seen a minute swelling, the base of the inferior larynx. Both trachea and bronchi are now hollow throughout.

The lungs, a little more voluminous, though still of the same form as the previous day, are raised higher on the ligament of the stomach, with which they are connected by a narrow slip detached from their superior margin. The cavity in the lower half of the lung is enlarged. In the upper half, still dense, are observed the rudiments of air-vessels, in the shape of numerous minute delicate short tubes, or rather solid cylinders, detached from the lower end of the bronchial tube. These rudiments may result from the consolidation of the gelatiniform substance of the lung in linear directions.

§ 13. While the trachea is lengthened during the following days, its cavity is equally enlarged in proportion to its *parietes*. It is with the trachea of the chick in this respect, as it is with the intestinal canal and all the tubular viscera of the vertebrated animals. At first it has for a long time thick walls in proportion to the cavity; and while the latter enlarges, the former are attenuated, when the soft gelatiniform matter of which they at first consist is insensibly hardened and consolidated. In the chick, the induration of the tissue of the trachea seems to begin on the eleventh day only, when the first traces of the *circles of the trachea* are visible.

I cannot say with certainty if the cartilaginous circles of the superior larynx are formed before the other circles of the trachea, because it is scarcely possible before the twelfth or thirteenth day to detach the superior larynx from the contiguous parts, without destroying it more or less completely. The cartilaginous circle of the inferior larynx, and at the same time the first circle of each bronchial tube, I can certainly show arise sooner than the other circles of the *trachea* and *bronchi*, since their rudiments are seen on the ninth day of incubation. In other respects the circle of the inferior larynx was at this time formed distinctly of two lateral parts or semicircles, which did not yet unite on the median line.

The formation of the other tracheal circles takes place by the gelatinous matter of the trachea being consolidated first in the lower wall of this organ, by transverse streaks, short, straight, and parallel. I have not, however, been able to determine if this consolidation takes place first in two different points on both sides of the median line, or only in one point of that line; but it has always appeared that consolidation proceeds directly from the median line, and thence continues on both sides. However it be, when each consolidating process separately has formed in the lower wall of the trachea a small transverse streak, they advance insensibly above on both sides, till the two extremities of the streaks meet, and are blended. In all instances the articulation of the trachea I saw commenced on the twelfth and thirteenth day, not properly on the lateral parts, but at the lower side, and proceeded to the superior, giving at this time the trachea of the chick a great resemblance to that of most mammalia.

The cartilaginous circles begin to ossify, when the chick approaches the term of maturity in the posterior part of the trachea, by the appearance of four equidistant ossific points, two in the upper, and two in the lower semicircle. As the upper points enlarge upwards, and the inferior downwards, it results that the two upper and the two lower points coalesce sooner than the lower and upper point of each side. In the the osprey, (*Aquila ossifraga,*) however, the ossification proceeds from two points only.

§ 14. At the instant the tracheal circles are developed, the mucous membrane seems also to be formed, and to acquire an independent existence within the trachea. Moderately thick at this time, it quickly becomes more so, and afterwards, in the process of consolidation, it is attenuated. Finally, during the last days of incubation it is closely attached to the cartilaginous rings and their fibrous intervals, though before it had been loosely united to the interior of the tube.

§ 15. From the seventh day the lung grows rapidly, its upper margin approaching the vertebral column; and when on the ninth or tenth day it touches this, the external side of the organ is applied still more closely to the ribs, and is united to them most intimately about the twelfth day. In all probability this union is effected at first between the two layers of the pleura mutually applied—the *pleura costalis* and *pleura pulmonalis*, though these are afterwards absorbed.

§ 16. The lungs are still united to the air-sacs. The growth of the former organs, at first greatest from the seventh to the twelfth day, after this period, advances at the same rate with the trunk. The greatest developement of the air-sacs is after this; but they increase then with such rapidity, that some days before the chick quits the egg, they envelope all the viscera of the chest and belly.

§ 17. The intimate structure of the lung of birds is best seen on the tenth and subsequent days, when the bronchial tube enters the lung, not by the anterior margin of the organ, but a little behind by the internal side. From this it transmits several radiating branches, forming a superior and inferior layer, the first turned to the back, the second to the abdominal cavity, and each branch of which is subdivided into smaller branches or twigs. From each of the principal branches, also, are detached innumerable short filiform tubes formed of a delicate membrane, which, without ramifying, are terminated by small globular swellings. These tubes, which are retained together by soft cellular tissue, and therefore easily separable, are all of the same length, and penetrate the substance of the lung; and hence the terminal swellings of the superior layer of branches communicate with those of the inferior layer. Besides these tubes, others in smaller number, issuing from the upper layer of bronchial branches, are directed with their swelling extremity to the back. After the period mentioned, the structure now described is no longer distinct, in consequence of the cellular tissue of the lung on the one hand becoming proportionally more abundant and more consolidated, and of the greater number of blood-vessels in the lung, by which the bronchial terminations are united.

§ 18. The insensible developement of this structure I have been unable to trace with accuracy. But it is matter of observation that the small tubes terminated by swellings appear only after the formation of the principal branches of the *bronchi*, that the swellings of these tubes appear first, and that the stalks of the vesicles are a subsequent formation.

§ 19. The *aeriferous sacs*, I have already remarked, appear to form originally in each lateral half of the embryo a simple bladder with gelatinous parietes proportionally thick, and enclosing one small cavity. If this view is well-founded, we might expect to see developed during the subsequent days in this bladder slightly elongated small partitions, which at length, when this cavity becomes longer, divide it into four small cavities, not directly communicating. In point of fact, I have observed on the ninth day of incubation, that this bladder, of which the *parietes* are still thick, and which is not only directed along the inferior or external margin of the lung, but exceeds this organ before and behind, contained a series of four cavities, separated from each other by partitions proportionally thick.

When during the following days these cavities, of which three are in the thorax, and one in the abdomen, enlarge, the enclosing wall insensibly becomes thin in every direction, and on each side there are formed four distinct bladders, lined by a thin continuation of peritonæum, attached to the vicinity of the lung. As these bladders increase in size, their proper membrane and the attached peritonæum by distension become so much thinner in proportion to their volume, that unless they are traced from the first, the observer might suppose that the air-sacs contained in the thorax and abdomen of birds are peculiar formations from the peritonæum,—a mistake committed by several naturalists who have confined their researches to the adult animal. From my observations on the mode in which the air-sacs are developed, it results that they do not contain in their cavities the viscera of the thorax and abdomen, as has been sometimes alleged, but that they merely penetrate the insterstices left between the organs, and that these *parietes* are applied immediately to the organs covered by the pericardium and peritonæum, so that they are exterior to the cavities of the air-sacs.

In proportion as the bladders communicating with each of the lungs enlarge, the contained fluid gradually disappears; that in the posterior bladder last; and at the same time there are formed in the interior of some of these bladders incomplete partitions, which divide them into several compartments. The posterior bladder on each side becomes at a later period the *great abdominal aeriferous sac*, while the two anterior bladders become the two *cardiac aeriferous sacs; (bulla cordis anterior et bulla cordis posterior.)* The communication of these sacs with the bones does not take place till some time after the chick is hatched.

(*To be concluded in next Number.*)

ART. VI.—*Observations on a form of Scarlet Fever succeeded by Rheumatic Affections, in Illustration of the West India Epidemic.* By ALEXANDER MURRAY, M. D., Aberdeen. In a Letter to one of the Editors.

DEAR SIR,

HAVING lately perused various descriptions of the remarkable eruptive epidemic which has recently prevailed so extensively in the West Indies, where it appears to be considered as a new disease, I feel a wish to direct your attention to certain points of similarity between that complaint and a form of scarlet fever, observed by me a few years ago in the upper part of Aberdeenshire, of which an account has been given in the seventeenth volume of the Edinburgh Medical Journal. Independently of its resemblance to the eruptive epidemic or "dandy fever" of the West Indies, this variety of scarlet fever appears not unworthy of notice, namely, for a painful swelling of the joints that took place *after convalescence was begun*, and indeed, in several instances, after the patient had got rid of every symptom, unless a slight remain of weakness. A similar affection of the joints often occurring after all previous symptoms had vanished for several weeks, appears the most remarkable feature in the foreign epidemic,—a disease of which the principal character appears to be, that, beginning with irregular symptoms of a febrile nature, these are succeeded by a cutaneous eruption, which after an indefinite period is followed by a rheumatic affection.

As the description of the particular form of scarlatina above alluded to, given in the seventeenth volume of the Edinburgh Medical Journal, consists only of a few lines, it may be here copied. " In the third variety, the symptoms of oppression existed in a high degree. The patient was seized with chillness, sometimes amounting to rigor, great feebleness and sickness, rarely inducing vomiting; the succeeding eruption was never complete, but appeared faint, or in the form of spots; the pulse, after a day or two, often intermitted remarkably, but was full. The pain of throat was trifling; but mucus collected here and in the mouth in great quantity, and was very troublesome. After a few days the tongue became dry, and sometimes sordes collected on the teeth, while not unfrequently a diarrhœa also took place and seemed injurious. In no instance of this form did death ensue. The pulse lost its intermission; the diarrhœa abated; the mouth regained its moistness; and the eruption was more fully evolved. Upon the occurrence of these symptoms recovery went gradually forward. It was always long before perfect restoration to health took place, and very often after convalescence was begun, a painful affection of the joints, especially in the wrists and fingers, gave rise to a renewal of fever, and retarded the recovery. Severe attacks of sickness, sometimes amounting in appearance to a faint, were common in the latter part of this form." (P. 343, &c.) In another part of the same paper I have said, " An affection of the joints was mentioned as not uncommon in the last stage of the disease. This was a swelling of the wrists and hands, less commonly a similar affection of the knee-joints and other articulations, attended with much pain and want of the power of motion. This in general went off in a few days; but in one instance seemed to be converted into a permanent paralysis."

The coincidence between this affection of the joints and the rheumatic attack which occurred in the last stage of the West India epidemic, is sufficiently obvious; but it may require being pointed out, that among four varieties of scarlet fever, the third, as above quoted, is the only one which was followed by this peculiar affection, while it has in several other respects a notable resemblance to the foreign eruptive fever. Thus Mr Nicholson mentions, that in the West Indies the rheumatic pains of the last stage did not usually occur, except in cases not attended with the eruption, or wherein it was not distinct;* and in my account it is said that the eruption was never complete. Again, no affection of the throat was in general observed in the West Indies, Dr Cock being the only person who saw the "dandy fever" accompanied with that symptom. In connection with this it is to be noticed, that in the Aberdeenshire cases the pain of throat is mentioned as trifling. Farther, one of the most extraordinary characters of the "dandy fever" is the very small number of fatal cases, notwithstanding the severity of the symptoms. In the same way, though the symptoms described by me had, as is obvious, a formidable appearance, no instance of death occurred. In the last place, it is to be gathered from Dr Cock's account, that the variety of fever accompanied by affection of the joints, was, according to his observation, confined to adults. For he observes, that "in children the third or rheumatic stage was wanting;"† and any one reading the whole account of scarlet fever in Aberdeenshire in your seventeenth volume, would observe that the portion of it formerly quoted relates to a form that was not observed in infants and children. It might be objected that the description of this variety of scarlatina will bear no minute comparison with the accounts from the West Indies; but these will be found to differ from mine scarcely more than they do from one another. In short, considering the interest attached to the "dandy fever;" and as one actual observer has declared his belief that it will prove a modification of scarlatina, it may deserve being known, that one form of undoubted scarlet fever has been already described, which was distinguished from other varieties of that disease—prevailing in the same place at the same time—by characters which bore no slight resemblance to those observed in the eruptive epidemic of the West Indies.

The circumstances which have been related are, I trust, enough to justify my interference with the subject. I am not, however, considering my imperfect means of forming a judgment, so presumptuous as to give my decided opinion about the nature of the West India eruptive epidemic; nor does it even appear fitting for me to offer any detailed argument on the subject. The following few general observations may, however, be introduced.

In the first place, there is in the accounts from the West Indies nothing incompatible with the opinion, that they ought to be referred to scarlet fever; for we have many descriptions, relating without doubt to scarlet fever, differing as essentially as these do from the common form of that disease. It is also clear that the eruption in the West Indies was not dissimilar in appearance to that which occurs in scarlatina; and the cuticle peeled off, as is usual in that disease. It may be added, that the extensive prevalence and slight mortality in the "dandy fever," would well enough correspond with what has sometimes been observed of scarlatina, as no disease whatever approaches more nearly to being universal in its attacks than it has occasionally done; nor is there perhaps any complaint more void of risk than scarlatina has occasionally been. It is indeed that disease, regarding one form of which was made the well-known observation, that it is dangerous only "*nimiâ medici diligentiâ.*" In the West Indies, when the eruption appeared, the symptoms underwent a marked aggravation. In the same way, when the rash appears in scarlet fever, the symptoms are not diminished, but rather increased; an occurrence which, so far as my observation or knowledge goes, has no parallel in any other eruptive contagious fever. The usual absence of affection of the throat in the West Indies does not prove much against the "dandy" being identical with scarlet fever, as more than one observer of the highest authority has considered sore throat no necessary constituent of the disease. I have myself seen various cases, and these mostly of a serious complexion, wherein the complaint in the throat was as trifling as possible. It ought not, however, to be forgotten, that Dr Cock met with sore throat in combination with the West India fever; and that Dr Stedman, in his "account of an anomalous disease which raged in the islands of St Thomas and Santa Cruz, &c." notices aphthæ and soreness of the mouth; for which, he says, alum and borax washes were prescribed with good effect.

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* Edinburgh Medical Journal, No. 98, p. 116.

† Ibid, No. 102, p. 45.

Observers of what is considered new or uncommon are apt to receive strong impressions, and their representations are consequently more highly coloured than future experience in general completely sanctions. I venture to suspect that the peculiarities of the late eruptive epidemic in the West Indies have been brought forward rather too prominently. For example, we would be led to believe, from some of the general descriptions, that excruciating pains of a rheumatic character, commencing sometimes many weeks after the disappearance of the eruption, formed a regular constituent of the disease; but let the facts be scrutinized, and it will be concluded, that those pains took place only in a limited number. Thus in St Thomas, "the negroes were rarely attacked with severe secondary disorder."* In Antigua, "in those cases in which the eruption was distinct, the pains usually vanished with it."[*] In St Bartholomew it was observed, that, "when the practice was sufficiently energetic, the after-pains constituting the rheumatic stage were generally prevented;"[†] and that "in children the third or rheumatic stage was wanting."[‡]

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* Edin. Med. Journal, No. 97, p. 233.

The form of scarlet fever which ends in a rheumatic condition, ought not to be confounded with that swelling of the face, hands, and feet, or that stiffness of the joints which occasionally occurs in *an early stage*. Such symptoms are occasionally met with in this country, and would seem to be still more common on the Continent.[§] The variety wherein severe pain of the joints occurs after health is almost completely restored, is probably but of rare occurrence. It may be said to have occurred at Alford, in Aberdeenshire; and I have an opinion that the West India eruptive epidemic ought also to be considered an example of it. Except these instances, I have not been able to meet with any distinct notice of this form, unless in one admirable dissertation on scarlatina, namely, that by Willan, in his work on the Skin, wherein he quotes from Doringius, (who wrote of scarlet fever as prevailing in Germany,) the following words. "*Terminatur* ad plurimum, translatione materiei ad articulos extremorum, cum tam dolorifico tumore ac rubore *qualis apud arthriticos esse solet*."[||] This author also quotes from Dr Sims the following words respecting scarlatina, which I transcribe, as they are particularly worthy of notice, on account of their similarity to some of the accounts from the West Indies. "Another circumstance in the months of November and December was, that a few days *after the apparent change* of the disorder a swelling attacked the face, but more frequently the extremities, attended with most excruciating pain. This swelling was neither red, puffy, nor œdematous, and did not depend on the previous mode of treatment as it occurred in those who had been treated in the most different manners. Some first complained of a violent toothach; after two or three days they complained of an equally violent pain in the back, the first one gradually subsiding. In a day or two more, or even sooner, the pain attacked their elbows, wrists, and hands, which were usually the parts last attacked. These pains in some patients were only in the hands and arms."*

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[*] Edin. Med. Journal, No. 98, 116.

[†] Ibidem, No. 102, p. 44.

[‡] Ibidem, p. 45.

[§] Thus Roche and Sanson, of the Hotel-Dieu, speaking of scarlatina in their late work called, "Nouveaux Elements de Pathologie Medico-Chirurgicale," use these words in describing shortly the eruptive symptoms, "Les mains et les pieds sont souvent gonflés, douloureux, et plus rouges que les autres parties du corps; la face et les paupieres sont quelquefois bouffies." I mention these words partly that they may be compared with some of the descriptions of the "Dandy" at *an early stage*, as with the following: "In some the features were swollen and distorted, especially the eyelids; in others the fingers suffered most in this way, &c."—(Ed. Journal, No. 97, p. 229.)

[||] Willan on Cutaneous Diseases, Vol. iii. p. 302.

It has now occurred to me, while employed with the foregoing remarks, that it is possible to specify three separate kinds of *superficial* swellings occurring in scarlet fever, which perhaps have not been sufficiently distinguished from one another. The first is a swelling in the hands, face, and feet, red, but not very painful, along with a stiffness of the whole surface, particularly over the joints. This, when it occurs, attends the eruption, and, as it may be viewed as simply the result of the cutaneous inflammation, it speedily disappears along with its cause. The next is an anasarcous swelling of the parts above-named, and being the effect of a general tendency to dropsy, it is to be treated accordingly. The third form is a swelling of the joints, always attended with great pain, and sometimes with redness, but for the most part without either œdema or redness. This, like the second form, takes place after other symptoms have disappeared; but it appears to be confined to the more robust adults, and to occasion little or no danger, not in general producing even much acceleration of the pulse; while, on the contrary, the anasarcous form attacks chiefly children, and is of a serious import. The third form has been styled rheumatic; but at Alford I found it in general much more tractable than true rheumatism usually is. I there found that this affection of the joints could commonly be removed by warm fomentations. In more severe cases antimonial powder, with a minute portion of calomel and opium, exciting diaphoresis, generally carried it off. Both the first and third forms of swelling, but with an unusually severe character, appear to have been met with in the West Indies.

Though the foreign eruptive fever should come to be received as scarlatina, it will always be viewed as a highly remarkable epidemic; nor will this be the first or second instance in which scarlet fever has passed for an unknown disease. Of this various proofs might be adduced; but the following words from Willan shall suffice. "The scarlatina is again described as a new disease, under the title of *Febris miliaris rubra*, which is said to have appeared at Leipsic about the middle of the seventeenth century," (Vol. iii. p. 302;) and again, "It is singular that this complaint, when epidemical in the vicinity of Leyden about the year 1699, should be again mentioned and described as an unknown disease," (p. 309.)

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* Willan on Cutaneous Diseases, Vol. iii. p. 267, &c.

So varying indeed are its forms, that it might be stated in a general way, without being far wide of the truth, that until comparatively recent times, wherever scarlatina appeared as an epidemic, it was received as an unknown disease, and described under a new name.

Though the main object of the present communication has now been fulfilled, I may be permitted yet to notice another peculiarity in scarlet fever, which is interesting and of practical importance, but probably not generally attended to or known. I allude to the great susceptibility to scarlatina that I have observed on the part of women recently delivered, and, if I mistake not, throughout the whole period of nursing; and to the high risk incurred by such individuals when attacked with the complaint. So strongly was I impressed with these opinions at a time when much exposed to the infection of scarlet fever, that I had the utmost reluctance to visit any female who was under the circumstances just mentioned, lest the disease might thus be indirectly communicated. In the paper already mentioned, I have noticed that there occurred "several instances of midwives, previously occupied with patients in scarlet fever, carrying the disease to women whom they attended in labour." (Edin. Med. Journal, Vol. xvii. p. 349.) And that, (p. 344,) "of six who died of the fourth form of scarlet fever, two were women a day or two after delivery, and other three women with infants at the breast." Indeed I feel convinced, with regard in particular to women after child-bearing, that they are readily infected, even by a party who has not undergone the disease, and that, if attacked, their chance of recovery is slender indeed.

I may be mistaken in supposing these points to have any novelty; but it is certain that they are not noticed by our ordinary medical authorities,—at least I have been able to meet with them only in the excellent work already more than once mentioned, where we have the following words. "Whenever the scarlatina is epidemical, it usually affects women two or three days after lying-in, and often proves fatal to them."* Willan also quotes the following words regarding the scarlatina at Leipsic, wherein the susceptibility and danger of women in the puerperal state are particularly mentioned: "Purpura in nostra Lipsiâ perquam frequens est, ut non modo pueros cum juvenibus, adultos cum senibus, viros cum fœminis, et *ex his speciatim puerperas* tota pene die invadat; et paucos intra dies, ni summa habeatur cura, interimat. Sed non a multis retro annis, hanc purpuræ molestam familiaritatem civitas nostra experta est, sed demum paulo ante annum hujus sæculi quinquagesimum, *cum*

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* Willan, Vol. iii. p. 305.

puerperarum quam plurimarum strage, primam ejus invasionem recordantur incolæ."*

It is perhaps worth adding, that at one time it occurred to me that the disease called puerperal fever might be in reality only a modification of scarlatina. This opinion, not, so far as I know, suggested before, appears on more mature consideration very doubtful; but even a *guess* may be admitted upon a subject, regarding which authorities differ so much from one another,—almost every one of these believing that he only truly understands the subject, while every other person has mistaken the nature of the disease, or perhaps never seen it, having confounded it with something similar. The opinion, that puerperal fever is no other than scarlatina, might be speciously supported by several circumstances, wherein the latter disease corresponds with the characters attributed to the former. For example, women after lying-in, when exposed to scarlet fever, are usually infected; and they may be so, as before observed, by persons not themselves diseased, while from an attack of this kind they seldom recover. In them, too, the disease is apt to be mistaken, the characteristic symptoms, viz. the cutaneous eruption and sore throat, being, so far as I have had an opportunity of observing, peculiarly indistinct. If it should be objected that pain of belly usually occurs in puerperal fever, it might be stated that abdominal symptoms also occur in the scarlatina after lying-in, being indeed probably common to all severe illnesses in women newly delivered. It is no conclusive objection to say, that upon this supposition, wherever we have scarlatina, there ought puerperal fever to be also; for it is perfectly possible that there might be something, operating occasionally throughout a wide tract, which renders puerperal women peculiarly susceptible to this infection at one time and not at another. Notwithstanding all this, the probability on the whole is, that puerperal fever is a disease *sui generis;* though other diseases, and among these scarlatina, may at times have been mistaken for it. Still it might deserve inquiry whether there has been any coincidence, in point of time and place, between the various appearances of puerperal fever and that of scarlatina. In Britain both are probably diseases of but new date. In the north of Scotland, the former seems to have made its first appearance in 1760; while in the same quarter scarlatina was, so far as I have ascertained, first described in "Observations on the Sore Throat and Fever in the north of Scotland, by Robert Saunders, M. D. Physician at Banff, 1777."

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* Willan, Vol. iii. p. 306.

To these observations you may attach whatever value you may see proper. They have extended far beyond the main object of this communication, which has thus become of a much greater length than was originally in view. Were they, however, to be considered of any even the smallest value, an apology for length would be unnecessary, as the subject is one of the most important in the wide range of medical science, and the field less exhausted than most others which could be pointed out for cultivation. All this will, I think, be readily admitted, upon considering how little is known about the intimate nature of scarlet fever; the occasional great mortality from the disease; and the many different shapes under which it has been known to present itself. Again and again, as already mentioned, has it been described as a new disease that had just sprung up. The difficulty of recognizing it does not apply to the epidemic form alone; but exists much more strongly in cases taken singly—as in distinguishing, when the complaint is not prevailing, an instance of scarlatina without any eruption, from common continued fever having, as sometimes happens, sore throat combined with it; or even from *cynanche tonsillaris*. Every one must have verified this remark, and it is at present made the more particularly, that I have very recently met with three cases, to which it exactly applies. I have observed that scarlet fever in adults is accompanied with more rigidity of the lower jaw, and more of a frothy discharge from the mouth, with less swelling of the uvula and tonsils, than is usually found in sore throat of any other description. Whenever a sore throat, however trifling, runs through a neighbourhood, or even a family; as a rule—liable no doubt to some exceptions—it may be laid down that the disease is scarlatina. The student of disease, who has only seen well marked cases of scarlet fever, will not anticipate much of the difficulty just contemplated; but on the present subject, as on many others, doubt, in place of being a mark of ignorance, will rather prove to be the result of extended experience; which has shown that there are other diseases to be met with, similar to the present, at times, in appearance, but in nature essentially different. Nothing, however, more readily procures confidence from others, than that air, of which doubt never forms a portion; nor is there any art resorted to by the empiric with more success, than the unhesitating demeanour which he so usually puts on.

For various reasons I entertain an opinion, that scarlatina—unlike our common continued fever—is more frequently prevalent, and comparatively far more fatal in country parts than in large towns. Farther, in a situation of the former kind, after considerable opportunity of observing the disease, I found it nearly as fre-

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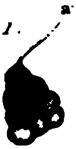


Fig. 2.

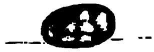
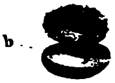


Fig. 3.



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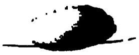

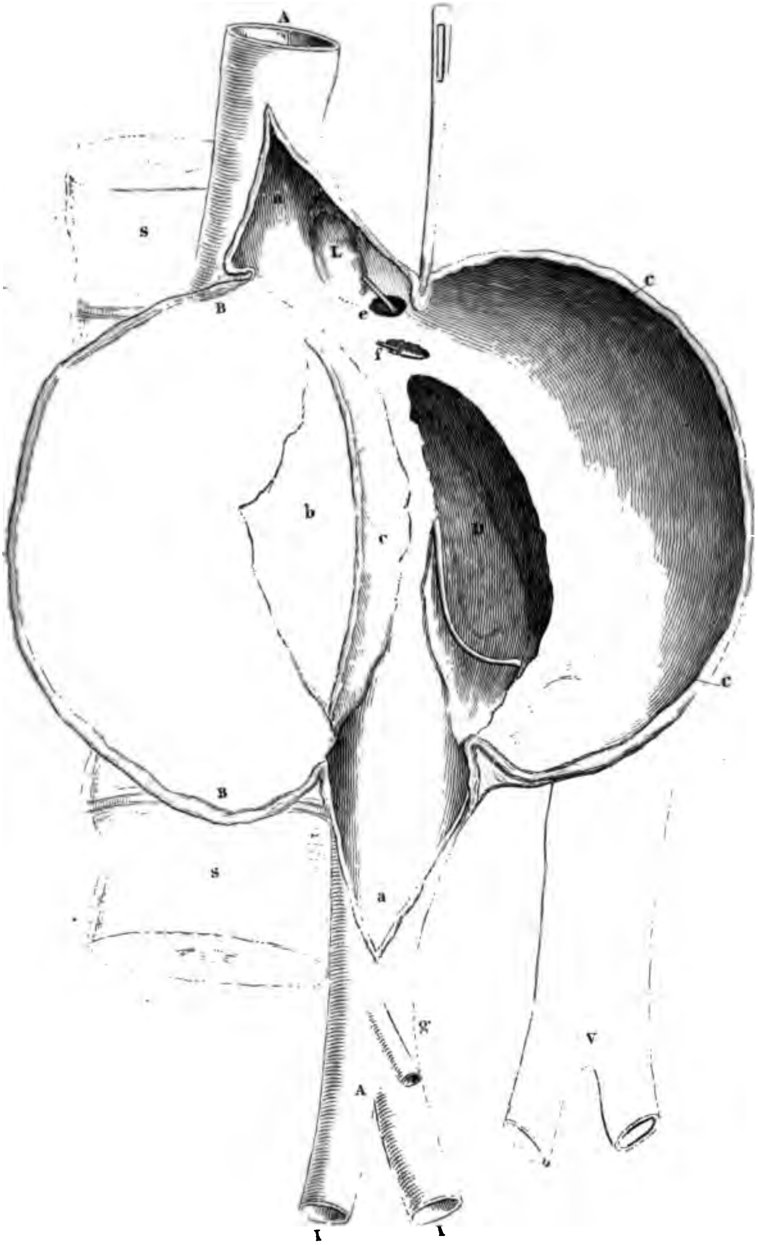

quent and fatal in adults as among infants and children; a fact very different from what has been observed by most other persons. I have also met with a few cases of scarlet fever, differing, I think, from any described form. The patient complained of nothing; was indifferent to all circumstances; and, on the whole, might be likened to one labouring under an overwhelming poison. So true is this, that I saw one patient whose illness turned out to be scarlatina, regarding whom the medical attendant entertained and expressed an opinion, that the symptoms were the result of a poison having been swallowed. In cases of this description, I fear the doom, from the very first hour of attack, may be said to be irrevocably fixed. Perhaps, indeed, it would not be easy to select a more legitimate subject, for the cautious trial of something new, than the worst form of scarlatina, as these cases, under the present system, barely admit a ray of hope. Some imperfect views of my own on this matter I do not here venture to propose.

In concluding these observations, which have been of rather a desultory description, I shall mention a case, which, if not unexampled in its symptoms, is at least very remarkable, and more particularly so, from the circumstance that recovery took place. I have not at present the means of giving in detail the symptoms or treatment, but the following brief outline may be depended on. A young man previously healthy, who had laboured for a few days under scarlatina of no alarming character, was rather suddenly seized with symptoms of general dropsy, that is, with œdema of the whole body, swelling of the belly, incessant cough, and total blindness, the urine being at the same time in very small quantity, though often evacuated. Under these circumstances, it may be fairly assumed, that at one and the same time the patient had, along with anasarca, dropsy of the head, chest, and belly. Yet all this went off in a few days, and, before the lapse of many weeks, health was completely restored.—Yours very respectfully,

Alex. Murray.

Aberdeen, 10*th February* 1830.

Art. VII.—*Case of Aneurism of the Abdominal Aorta.* Reported by R. Spittal, Esq., one of the House-Surgeons of the Royal Infirmary.

THE subject of the following case was a patient in the Royal Infirmary of Edinburgh for many months, under the care of Drs Christison and Gregory, physicians to that institution.

The case is interesting both in a physiological and pathological point of view.

P. K., a man, aged 35, June 27, 1829. Has been addicted to the abuse of spirits, and has twice had an attack of delirium tremens. The report of this date is to the following effect: Complains of pain of the loins, stretching to the epigastrium, where there is a circumscribed pulsating tumour occupying part of this region and the left hypochondrium about three inches in length. He states that the tumour has existed and pulsated for four months.

29th. The pains of the back and abdomen continue.

July 2d. Pain of the right side of abdomen, with tenderness. Tumour perceptible to the right of epigastrium.

August 22d. Complains much of general pain of abdomen, and of diarrhœa.

September 17th. Very slight "bruit de soufflet" heard over the most anterior part of the tumour, immediately below the ensiform cartilage, and a little to the left side. Diarrhœa abated.

December 25th, Pain of the side in the region of the tumour has for three days past been more severe.

January 18th, 1830. Has been for some days liable to fits of severe pain in the region of the tumour.

February 1st. Increased pain of the tumour, with vomiting of ingesta and singultus.

3d. Gradually sunk and died this morning.

Such are occasional reports made concerning him during his residence in the hospital. He was seldom if ever entirely free from pain or uneasiness in the region of the tumour, especially towards its left side. The tumour itself was very tender on pressure generally; its pulsation was always distinct; and when grasped by the fingers and thumb, was found to distend in every direction. The pulsation, however, was more evident at the date of his admission and for some time afterwards than a month or two before death, and the tumour appeared to have diminished in size. It certainly had not increased for some time before that event took place.

The stethoscope was frequently applied; but no very distinct "bruit de soufflet" ever was heard.

From the time this patient entered the hospital, until the day of his death, his bowels were for the most part constipated, in general requiring a powerful cathartic to move them.

He found occasional ease by sitting bent forwards in bed, and when up, he always walked with his body bent in this position; he likewise lay in the same manner.

Examination, February 4th.—The inner and middle tunics of the aorta were diseased in the usual manner, with here and there small calcareous depositions. In consequence of this affection, the inner surface of the aorta was uneven and somewhat corrugated. The arch of the aorta was not dilated, nor were the semilunar valves diseased.

Immediately above the part at which the cæliac and superior mesenteric arteries are given off, there was a small oblong dilatation of the aorta,—or, in other words, a small aneurismal pouch about half an inch in length, and projecting anteriorly. (L.) Also just below the origins of the vessels mentioned, occupying the space between these, and to about one-and-a-half inch of the inferior mesenteric artery, there was a large aneurismal tumour consisting of two globular sacs attached to each other. The one, which was large and firm, and about the size of a goose's egg, adhered to the vertebral column, and had effected partial absorption of one of the bodies of the vertebræ, and projected from the right side of the aorta. The other tumour was more anterior, smaller, softer, and projected from the large tumour over to the left side.

The aneurism in its enlargement had thrust aside all the adjacent soft parts. The *vena cava* lay over the larger tumour, being firmly attached by means of the left renal vein, which crossed over the smaller tumour, and compressed it, having caused slight projection on each side. The left renal vein, where it adhered to the tumour was nearly obliterated, and would scarcely admit at one point a small probe. The right renal vein was quite natural.

The left renal artery was found to go off from the aneurism in the space between the larger and smaller tumours on the left side. The right renal artery arose from the anterior surface of the large tumour; it was much about the usual size, but could not be traced beyond the walls of the tumour, being there quite obliterated.

The aneurism, when examined internally, presented the following appearances:—The larger tumour was found entirely filled with solid contents. Externally with regular and distinct layers of fibrin, of a pale muscular aspect, (B, B.) Internally, or towards the course of the aorta, there were clots of recently coagulated blood of a dark colour, and comparatively soft consistency, (b.) These occupied about somewhat less than one-fifth of the whole tumour, and were enclosed on their aortal side by a pale and apparently older layer of fibrin, over which the blood had flowed during life (c); it was very smooth, and was attached to the remaining portion of the aorta, (a, a.)

The recently coagulated blood in the large tumour communicated with recent coagula in the smaller, where a little fluid blood was likewise found. There was a considerable fibrinous deposition on the inner surface of the small tumour surrounding the recent coagula. Immediately below the edge of this deposition, in contact with the remaining aortic surface, the left renal artery was found to arise (h); and it is probable that a communication existed here, between the blood flowing in the course of the aorta and the recently coagulated blood described as found in the small tumour, which probably was fluid shortly before death.

In the large tumour of the aneurism the middle and external coats of the aorta were observed dilated, but the inner insensibly became lost as it was traced from the remaining portion of comparatively natural aortic structure into the tumour.

The smaller tumour was not so minutely examined; but it probably possessed the same structure.

The inner surface of the little pouch was irregular, and much more diseased than the aorta generally.

This aneurism then, was in a manner double, and probably arose from two pouches similar to the small one described.

At the angle of junction of the two tumours, there was a portion of comparatively natural aortic structure dividing these; and between that portion of the aorta lying over the vertebral column, and the two tumours, there was a free communication from the upper to the lower part of the aorta, by which during life the blood had flowed.

The right kidney was very small, being about one-fourth the usual size, while the left was perhaps rather above the usual dimensions.

The case described illustrates well the process of natural cure which is found to take place in aneurism. The large tumour was probably older than the smaller. The former contained more and firmer layers of fibrin than the latter, which probably during life contained a considerable quantity of fluid blood, besides the fibrinous deposition mentioned.

The anomalous pains which he experienced are readily explained by the pressure of the aneurism on the surrounding parts; and the constipated state of his bowels was in all probability caused by this, exerted upon the nerves belonging to the semilunar ganglia and solar plexus, some of which, indeed, were found stretched across the tumour.

As already mentioned, there was no satisfactory stethoscopical indication; and this is easily explained. In one report, the "bruit de soufflet" is stated as indistinct; but it latterly was quite absent. The cause of this seems to have been the extensive fibrinous deposition within the aneurism, which at first, when less extensive, permitted the sound of the motion of the blood into the tumour to be heard indistinctly, but which, by gradual increase of the deposition, prevented this from being heard latterly.

The most uncommon fact connected with the case, however, is the state of the kidneys, in reference to the condition of their vessels. It has already been stated that the right kidney was about one-fourth the dimensions of the left; and it is curious to observe, in connection with this fact, that the right renal artery was obliterated, the right renal vein being quite pervious; so that the kidney derived but a very scanty supply of blood, probably from the capsular artery, or those going to the surrounding fat, and this found an easy return by the vein. On the other hand, the left renal artery was quite natural, while the vein was nearly obliterated. Thus it appears that the small supply of blood which the right kidney received had a very marked effect upon its growth. The quantity of nourishment afforded was insufficient to repair the constant waste. The small dimensions of the right kidney may, however, in part be attributed to the loss of nervous influence, caused by the pressure of the aneurism on the nerves going to it. This, however, was not minutely ascertained.

Explanation of the Plate—AA, The Aorta; II, the Iliacs; SS, the Vertebræ; CC, anterior part of the large aneurismal sac emptied of the coagula; D, opening communicating with the small sac; e, the cæliac artery; f, the superior mesenteric; g, the inferior mesenteric; V, the vena cava.

ART. VIII.—*Cases of Painful Subcutaneous Tubercle.* By DAVID C. CARRUTHERS, Esq. Surgeon, Dundee, Member of the Royal Medical and Royal Physical Societies of Edinburgh, &c.

IN addition to the many interesting cases brought forward in the "Observations, &c. by William Wood, Esq." I beg leave to subjoin the following, which have occurred since my perusal of his first publication. These cases were taken with no view of publication, and on that account are in many respects deficient.

CASE I.—William Wilson, æt. 52, labourer, Balmossie, near Dundee, has a small subcutaneous tumour, situated upon the middle, posterior, and ulnar side of his left fore-arm. A slight elevation is observed in certain positions only, and between two veins of considerable size, having much the appearance of a varix. Its feel is peculiar, appearing to consist of fatty substance, and a small round body exceedingly hard, resembling a small shot, and evading the touch. It is the source of great uneasiness while working, and is particularly painful during damp weather; also in the night often causing him to awake, occasioned, he imagines, by the bed-clothes coming in contact with it, as it is exceedingly sensible when in the least compressed. Pain darts along the inner side of the arm to the arm-pit and shoulder. About sixteen years ago he was led to observe it on feeling a slight degree of pain near its seat. He does not think it has grown much since, nor can he trace it to any blow, prick, or other cause.

I removed it by two semielliptical incisions, including the smallest possible portion of integuments, and avoiding the veins on each side. It was seated somewhat deeply, adhering to the fascia beneath, but otherwise surrounded with cellular substance. A small subcutaneous branch of a nerve seemed to pierce its upper and inner part, but its exit could not be seen. The cellular substance surrounding it appeared more compact than natural. It was easily extracted without wounding either of the veins. The patient said the pain of cutting really was less than I had occasioned him while examining it some days before. After its removal I examined it very carefully. It was of the triangular appearance represented in the Plate Fig. 1. and of a dark brownish colour, lobulated; and at one of its corners B was felt and seen the small round hard body.

On laying it open by an incision through its longest diameter, it appeared to consist of a reticulated filamentous structure, surrounded by a tough membranous capsule, and containing a dark brownish fluid, staining linen and white paper of a coffee colour; the small, round, hard body consisted apparently of a cartilaginous substance, with a dark centre, and united firmly to the general mass. Several other parts of the mass were evidently acquiring a hard consistence, and approaching to the roundish form. No nerve could be traced within its substance. Once or twice, with assistance of the microscope, I could trace a small filament, but rather think it was only part of its reticulated structure.

Whether the nervous twig (a,) seen at its upper and inner part pierced, but was so interwoven with its internal structure that no trace of it could be detected, or that the external membranous covering was the distended *neurilema*, it is difficult to determine; but from the nervous twig gradually enlarging at its approach to the small tumour, it would seem to favour the opinion of Jacobi, " that they consist of a morbid change of the neurilema by deposition of a dark-coloured albuminous matter in the neurilematic interstices."

The small wound showed at first little tendency to heal, and its lips were for some time surrounded by an erysipelatous blush; but by the usual remedies at last it healed kindly. Nothing of the kind exists in any other part of his body.—September 1828.

Case II.—James Herd, æt. 49, feuar, Forthill, Broughty Ferry, neighbourhood of Dundee, has a small subcutaneous tubercle situated on his left arm, a little above the elbow, and at the outer side, a slight elevation is visible, and feels of a firm consistence, rather loosely attached in its seat, about the size of a coffee-bean, and immediately under the integuments, which are of the natural appearance. Complains of its being subject to paroxysms of acute pain resembling those of severe toothach, leaving it very tender to the touch for some time after. The paroxysms generally occur during the night, causing him to awake, and continue more or less severe for upwards of an hour The pain is seated chiefly in the tubercle, but extends also to the neighbouring parts. It is the seat of excruciating pain if accidentally struck, but it is noways painful when touched unless during a paroxysm. Is quite certain that the changes of the weather have a very marked effect upon it. The paroxysms are at present frequently brought on while working, by his having to throw up rubbish from a well, at which times they take place suddenly, and are of short duration, while at all other times they come on more gradually.

About six years ago he first observed it on feeling frequent stinging pains near its seat. There was then no prominence, but it could be felt of the size of a barleycorn, and moveable. Since that time it has slowly increased to its present size, and the paroxysms have likewise increased in frequency and in severity.

I removed it by two semielliptical incisions, including a very small portion of integuments; it did not adhere to the integuments; and was easily extracted from its bed of cellular substance; no vessel nor nerve could be detected entering its substance. Its size and form are represented in the Plate, Fig 2, much lobulated, of a cartilaginous appearance, and very hard. I divided it through its longest diameter, and laid open a small triangular cavity (b,) Fig. 3, having the internal surface of its parietes smooth, and moistened by a minute quantity of a glairy fluid. The small tubercle was evidently of a cartilaginous nature, covered externally by a very delicate film of condensed cellular web, destroying the transparency which it otherwise would have had, judging from the internal appearance.

The lips of the small wound healed by the first intention in the space of a few days leaving little or no scar. October 1828.

Case III.—A married woman, æt. 44, Dundee, who had a subcutaneous tubercle situated upon the upper and outer part of the leg, about a handbreadth below the knee. She had been troubled with symptoms differing little but in situation from Case II. for upwards of nine years. It was deeply imbedded in cellular substance, but was easily extracted by Dr Ramsay, with whom I examined it immediately afterwards. It consisted of a tubercle of the size and figure represented in the Plate, Fig. 4, of a hard fibro-cartilaginous nature, and when divided it was semitransparent and solid throughout. Neither vessels nor nerves were observed while extracting, and none afterwards could be traced into its substance.

It is pleasing to remark, in the above three cases, that none of their distressing symptoms have ever as yet threatened to return; and it is now rather more than four years since Case III. was operated upon.

ART. IX.—*Case of extreme unsusceptibility to Uterine Action.* By WILLIAM CAMPBELL, M. D. Lecturer on the Practice of Physic and Midwifery, &c. &c.

A. H. æt. 28, rather below the middle stature, but apparently well formed, came from the country in 1827 to be under my care during her confinement, owing to her having been on every former occasion delivered with great difficulty. When about three years of age, she received upon her spine an injury, by which her growth was for a very considerable period suspended.

Previous to making application to me, she had been five times delivered by the crotchet. In her three latter pregnancies, every attempt had been made by several practitioners in the quarter where she resided to induce premature labour, but without success. February 7th, the day of her arrival in Edinburgh, she was examined *per vaginam*, when the pelvis was found to be very deep, which rendered it difficult to determine the nature of the deformity, which proved to be a considerable projection of the promontory of the sacrum, towards the *os pubis*. She was ordered a dose of the compound powder of jalap. February 8th, medicine operated freely. The hand, warmed and anointed, was introduced into the pelvis; the index finger advanced through the *os uteri*, and the membranes separated from the substance of this organ for about an inch and a half upwards. During this proceeding the patient strained violently, and became extremely restless, when something was felt to have given way within; and this sensation was immediately followed by a discharge of about five or six ounces of aqueous fluid, which was thought to have been the *liquor amnii*, but which must have been a collection of water between the *chorion* and *amnion*, as the latter membrane was afterwards found entire on the accession of labour.

February 9th. Patient had nausea, with occasional uneasiness in the region of the sacrum, simulating that produced by commencing uterine action. *Pulv. Jalap. c.* ℈*i.* 10th. No discharge per vaginam since yesterday; and the pain in the sacrum has subsided. 12th, *Pulv. Jalap. c.* ℈ *iv. Submur. Hyd. gr. iv. M.* 13th. The hand was again introduced into the pelvis, and a further separation of the membranes effected, but without producing uterine action, or giving rise, as formerly, to any effusion, although the woman complained of the extreme suffering with which this proceeding was attended. On the 15th and 16th, the cathartic, as prescribed on the 12th, was repeated. The patient was directed to take exercise on foot daily.

February 20th. As the patient was within three weeks and three days of having completed the full term of gestation, and as there was no appearance of uterine action being established, it was determined to effect a greater separation of the membranes from the uterus than could be accomplished by the mere introduction of the finger. With this intention a male catheter, with its curve slightly diminished, was introduced to the extent of about three inches within the os uteri, and by a circular motion carried completely round between the membranes and the uterus. The instrument occasioned no pain to the patient, and when withdrawn, it was charged with a viscid sanguineous matter. At the same visit, with a view to allay pain in the side, and favour the accession of uterine action, she was bled to the amount of twenty ounces.

21st. Labour commenced at two P. M., and continued uninterruptedly until seven next morning, when I found the *os uteri* fully dilated, the amnion entire, and the breech presenting. The delivery was accomplished at half-past eight. The fœtus was still-born. No pulsation could be distinguished either in the funis or in the heart. In three quarters of an hour animation was restored by inflating the lungs, and injecting twice into the intestines, ℥iij. warm water and ʒij. whisky. Two tea-spoonsful of strong toddy were given by the mouth; the face and body rubbed with *Aq. Ammon.* At noon of this day, and at five in the afternoon, the infant vomited; and at half-past one on the morning of the 23d, expired in convulsions, produced in all probability by the pressure to which it had been exposed in its transit, and the strong efforts which were required in extracting the head; for the fœtus was considerably larger than could have been expected. In three hours after delivery, the woman had a severe rigor followed by unequivocal symptoms of peritonitis, which were subdued by a bleeding of ℥xxvj., warm enemata, and a large dose of the sedative solution of opium; and from this period she required no treatment.

In May 1828, this woman became my patient a second time, and required the same means as formerly. When labour was induced on this occasion, she was about seven months and a half pregnant; and, so far from the uterus being more easily excited into action as might have been expected, the difficulty, on the contrary, proved greater than formerly, for the catheter was required three different times. In this delivery, although the fœtus was small, yet the expulsive process occupied a period of twenty-three hours, of which seven hours were required for the transit of the head through the pelvis, showing the contracted state of this cavity. Animation, however, was not suspended; and the infant continued not only to live, but to perform all its functions regularly for nearly six days, when it was cut off by convulsions, of which the cause was afterwards but too obvious. On examining the cranium after death, ecchymoses were discovered at different points, and fracture of both the parietal bones.

The principal feature in the foregoing case is the great unsusceptibility of the uterus to be excited into action, affording an extraordinary instance of the influence of idyosyncrasy on an organ, than which, there is none, generally speaking, more susceptible of the effects of irritation, whether from mental or mechanical causes, more especially in the gravid state. It is much more prone to assume the expulsive action in a female who has given birth to several children, as also in those in whom its contents have been once or twice prematurely expelled, than in an individual who is pregnant for the first time, or in one who has never had a premature labour. I have known a young unmarried woman, in her first pregnancy, most violently salivated; and another, under similar circumstances, brutally kicked and struck about the abdomen,—in both instances, with a view to occasion premature uterine action, without, however, the least tendency to it being induced in either; while in a female, after several pregnancies, and more particularly one or more abortions, the slightest mental emotion, or the trifling accident of the foot slipping one step of a stair, are very common causes of premature labour. But the further gestation is advanced, the less likely is the process to be disturbed in first or subsequent pregnancies, by any cause.

Although we find in the earliest records of our own profession the induction of the premature expulsion of the uterine contents sanctioned, and in the history of some of the nations of antiquity not only sanctioned but even enforced by law, yet its application for the preservation of the young of our race is of modern adoption. Like other improvements in science, the practice had long to struggle with the superstition of the times, and the influence of the priesthood, from whose trammels, in some countries at least, it is scarcely yet completely emancipated. It was not until 1756 that the induction of premature labour was resorted to as a salutary measure for females with diminished capacity of the pelvis, when the operation was performed by Dr Macauley of London; and his success on the occasion speedily contributed to bring this invaluable practice into repute. When, in a female with deformed pelvis, a breech presentation is known to exist, this should be a farther inducement for resorting to the operation; since, were the fœtus in this position suffered to remain *in utero* until gestation were completed, the crotchet would be useless, and the section of the abdominal parietes would then be indispensable: whereas, had the head instead of the breech been placed towards the brim of the pelvis, the space might be adequate to the effectual application of the crotchet with safety to the parent.

The induction of premature labour has proved a salutary resource under several other circumstances besides diminished capacity of the pelvis. In a case where there is so great a degree of irritability of stomach in the early months, as to lead to apprehension lest the patient might sink from inanition, from her inability to retain nourishment, premature labour may be required; as also in an individual who is frequently threatened with convulsions. Dr Denman speaks of cases which he met with, where the fœtus always died *in utero* at a certain period of gestation, and where, by inducing labour before that particular time, he succeeded oftener than once in preserving life. From all I have heard or read, this operation is in itself perfectly safe; but when any formidable disease exists which cannot be relieved by premature labour being induced, or aggravated by suffering gestation to proceed, a practitioner should be cautious in resorting to it.

Though by this operation we secure the parent from being obliged to submit to the Cæsarean section, or the use of the crotchet, yet we cannot in any instance predict its results, in so far as the fœtus is concerned; first, because it is impossible in the living state to determine with much precision the actual dimensions of the pelvis; secondly, because we cannot always fix the date of conception; and, thirdly, because the size of the fœtal cranium, even in the progeny of the same parent, may vary in every one of a given number of infants. Generally, a woman suffers more with a male than a female child; but in this even the reverse occasionally happens, particularly where the short diameter of the brim is at all defective; for the cranium of a female fœtus is rounder posteriorly and towards the sides, whereas that of a male is flatter at those points.

Unless the pelvis be so capacious as to admit, without long-continued pressure, the transit of a fœtus that has completed seven calendar months *in utero*, the induction of premature labour can be useful to the parent only. For of all the cases which have come to my knowledge where the fœtus was reared, when supposed to have been born before the conclusion of the seventh month, I know of one only properly authenticated. In this instance the father is a professional gentleman, and, from particular circumstances, he can vouch for the accuracy of dates. It comes to be a question, then, what are the smallest dimensions of the pelvis that would justify us in allowing gestation to advance to such a period as to render it probable that the infant might be reared after birth?

Masters of the art have asserted, that infants brought to maturity have passed alive through pelvises of only three inches short diameter; but to such marvellous assertions I feel it difficult to subscribe, unless it be acknowledged that such infants were of dwarfish formation, which, however, does not appear to have been the case. Generally speaking, French children must be smaller than ours, since it is stated by the late Baudelocque and other respectable Continental writers, that their infants very rarely weigh nine pounds at birth; yet Baudelocque himself, who was a model of integrity, found in his experiments that he could not by forceps applied in a still-born fœtus compress the cranium in its transverse or parietal diameter to half an inch, without rendering the instrument straight by the effort, and bursting the integuments, and squeezing out some of the brain in the attempt. I therefore submit, that, under ordinary circumstances, a space of 3½ inches will be required in the short diameter of the *brim* for the safe transit of a fœtus come to maturity; but even where there is such an opening, I am sure practitioners of experience will grant, that occasionally the crotchet will be required, and more especially in a primary labour. It may be presumed, therefore, when the short diameter of the brim is considerably below three inches, or say 2½ only, that the induction of premature labour cannot be depended on as a salutary resource for the fœtus, unless it be unusually small. If a practitioner whose fingers do not exceed the usual dimensions in thickness, can insinuate, side to side, the four fingers through the brim until the point of the little one be on a level with the margin of this aperture, the fœtus may be allowed to remain *in utero* until the conclusion of the seventh month, and uterine action may then be induced with every prospect of success; provided always, however, that there be no deficiency of space at the *outlet*.

In conducting the latter investigation, we should caution the patient against violent straining or restlessness, lest the membranes might be ruptured, as the fœtus would then be exposed to the contractions of the uterus, which have too frequently, I suspect, led to its destruction. In some recent publications on midwifery, where this subject is treated of, I find, indeed, the puncture of the membranes recommended as a mean to induce premature labour, when their separation from the uterus has failed to have this effect. Since water, however, is almost incompressible, and since its presence is desirable to preserve the fœtus during its transit from the pressure of the surrounding parietes and parts, the membranes must, if possible, be preserved entire until the head is about to make its exit from the vagina. The foregoing case proves, that in the most insusceptible state of the uterus, the catheter used in the manner therein described, will supersede the necessity of puncturing the membranes; and, to avoid such an occurrence, the largest sized instrument should be employed. When the *os uteri* is quite impervious, and requires to be dilated, this should be done with caution; and whether the separation of the membranes is to be effected by the finger of the practitioner or the catheter, the depth of the pelvis, and distance of the os uteri from the vulva, must determine. When the uterine aperture can be reached by the finger, without the introduction of the whole hand into the vagina, this practice, as it is the safest, must certainly be preferred; but when the os uteri cannot be satisfactorily commanded by the index, the preference must be given to the catheter, since the introduction of the whole hand is a most painful practice. It will be seen from what has already been stated, that the time which may elapse before labour comes on, after the operation has been resorted to, cannot be determined; but generally, this happens in from three to seven days. In the meantime, however, the patient, with a view to accelerate this process, should take a dose of some laxative medicine every alternate day, and foot exercise daily. A wet nurse should be in readiness before hand; the infant should have suck frequently, but little at a time, and must be kept very warm. Under the best management, however, infants born prematurely are not easily reared. Many of them are cut off during dentition, or by the exanthemata.

ART. X.—*Remarks on the Ancient Principles of Toxicology.* By FRANCIS ADAMS, Esq. Surgeon, Kincardineshire.

FEW departments of medical science have been cultivated of late years with so much ardour as Toxicology, which indeed is a subject not only interesting of itself as treating of some of the most important cases which come under the cognizance of the profession, but possesses an additional attraction from the light which it has shed upon various parts of physiology and the practice of physic. It is not then to be wondered at that the late accurate and judicious publication of Professor Christison should have attracted no ordinary a degree of attention, as being the production of one who has long been looked up to as the highest authority in this country upon all matters relative to this science.

This work, the character of which may be said to be already fully established, it would be vain to blame and useless to praise,—nor is it for either of these purposes that I have undertaken to write upon this subject, but with the hope of contributing something to the improvement of it, by contrasting and comparing opinions lately advanced with those of a certain class of authorities which appear to me to be treated with undeserved neglect at present. In cultivating an acquaintance with any science or art I have always found it of advantage to connect distant with present opinions, and when they disagree, to balance the evidence on either side, *sine irâ aut studio, quorum causas procul habeo.*

Not to waste time with preliminary observations, I proceed to state, that the earliest author on toxicology whose works have descended to us is Nicander, who was physician to Attalus, the last king of Bithynia, under whose patronage he is said to have enjoyed superior advantages of acquiring a knowledge of poisons, by performing experiments upon condemned malefactors. He divides toxicology into two departments, namely, Theriacs, which treats of the bites of venomous animals, and Alexipharmics, which treats of deleterious substances swallowed by the mouth. This division is convenient, and acccordingly it was adopted generally by all subsequent authors of ancient times, as Dioscorides, Galen, Aetius, Paulus Ægineta, Avicenna, and Rhases. The Theriacs now possess comparatively little interest in this country, where scarcely any venomous snakes exist, and accordingly Professor Christison, in the work alluded to, has devoted only one short chapter to the consideration of this part. I shall endeavour to be as brief as possible in expounding the ancient doctrines.

The general remedies recommended by Nicander for the bites of all venomous animals are, sucking the wound, applying cupping instruments to it, and afterwards stimulant medicines, hot irons, and leeches. I question if modern science with all her vaunted superiority can make any great improvement upon this method of treatment; and therefore I have always wondered why the late Dr Cullen, in his work on the Materia Medica, should have spoken so disrespectfully as he has done of the practice recommended by the Colophonian poet. Respecting the method of sucking the wound he gives an important injunction, which would seem to imply, that he was not unacquainted with a physiological fact, which is generally supposed to have been lately discovered for the first time. He directs that the person who sucks the wound should not be fasting, from which it may be inferred, that he was aware that the vessels absorb most readily when in an empty state. This doctrine, which was revived a few years ago by M. Magendie, the ancients maintained most distinctly; and numerous allusions to it are to be found in the works of Galen, Paulus Ægineta, Avicenna, Avenzoar, and Averrhoes. It was probably with such views of the animal economy that Nicander has altogether omitted to mention general bleeding, and that the subsequent authorities on toxicology, who enumerate venesection among their remedial measures, such as Paulus Ægineta among the Greeks, and Avicenna among the Arabians, expressly inculcate that it is only to be had recourse to when the poison is distributed over the system. On this point Orfila and Christison have both omitted to declare their sentiments, but Dr Paris has done so in his Pharmacology, very properly forbidding venesection at the commencement, because it will only promote absorption. No practical point in the treatment of poisoning is more important than this; it is therefore agreeable to find, that the opinions recently advanced, and not yet, I believe, generally established, have the sanction of preceding ages to support them; and the satisfaction which this must afford M. Magendie and Dr Paris will more than counterbalance any regret which they might otherwise feel at finding, that what had at first been looked upon as a discovery, had proved but the revival of a forgotten truth. Upon this matter Celsus briefly expresses sentiments similar to those of Nicander:—*Omnis serpentis ictum et jejuni et jejuno magis nocere.* And in like manner Avicenna says, that when the veins are full the poison cannot get admission into them. How well this truth was understood 300 years ago, when ancient authority was in greater esteem than at present, may be estimated from the following quotations from Gorræus' Preliminary Dissertation on the Alexipharmics of Nicander:—"Nec verò id tantum incommodi habet fames, sed in multo majus periculum adducit hominem, quando et venæ plurimum exinanitæ et cibum vehementer appetentes, venenum avidius ad se pertrahunt, et in intima viscera cordisque arcem immittunt." On the effects of venesection he remarks, "Quod si quis etiam abundare videatur, et nihil eorum repugnet quæ sunt in venæsectione observanda, sanguinem audacter mittemus, *non quidem per initia (sic enim in venas deleteria traherentur* a quibus omni studio atque industriâ excludi debent) sed post vomitus alvique dejectiones, &c."

Nicander's particular remedies are mostly of a calefacient nature, such as birthwort, alkanet, and the theriac of vipers, which appears to have been just coming into practice in his days, but afterwards became the most celebrated of all the ancient antidotes. Galen among the Greeks, and Averrhoes among the Arabians, have written treatises expressly in praise of this composition, which, although now expelled from British Pharmacopæia, is still retained in several of the Continental ones. By Orfila it is frequently recommended. It was formed from vipers prepared along with a great variety of aromatic, calefacient, and acrid vegetables, such as pepper, cinnamon, ginger, myrrh, iris, anise, gentian, &c.

Celsus recommends nearly the same general remedies as Nicander. In the first place, he directs to apply a ligature round the limb, but not too tightly, for fear of occasioning torpor; then to extract the poison either by sucking, or by a cupping-instrument, along with scarifications. Emetics are likewise recommended, probably with the intention of expelling the poison from the system by the concussion which they produce. It seems to have been with this view that many of the ancient authorities direct to administer white hellebore for the cure of hydrophobia. For local applications, Celsus recommends the body of a hen or kid recently killed, and still warm, or some hot plaster, such as the Ephesian, which contained turpentine, frankincense, vermilion, wax, &c. As an internal remedy he prescribes the use of wine, with pepper or any other article of a heating nature, because, says he, *maxima pars venenorum frigore interimit.* The last remark is calculated to unfold the principles upon which most of the ancient antidotes were composed, for the poison was supposed to oppress the vital heat, and the remedies were chosen with the intention of supporting it. We will treat more fully of this doctrine afterwards.

Dioscorides, however, is the author to whom theriacs are under the greatest obligations, for he expounded the rules and principles of treatment so fully, that very few improvements were added to them in ancient times; and some will perhaps be disposed to question whether our present state of knowledge be upon the whole much superior. Most of his remedies are indeed derived from Nicander; but he has great merit in the judgment with which he has reduced them to first principles, and laid down rules for their application. Like Nicander, he directs that the person who sucks a poisoned wound be not fasting, and that he should keep some oil in his mouth. The part is then to be fomented with a sponge, and scarified or cut out.

And here I cannot help expressing my surprise, that Sir Everard Home should have been so little acquainted with the earlier records of surgery as to assign to his friend, Mr John Hunter, the merit of having first recommended excision of the bitten part as the best method of preventing hydrophobia. Burning with fire is another remedy which Dioscorides strongly recommends, stating that it proves efficacious, partly by subduing and dissipating the poison, and partly by forming an issue, which serves as an outlet to it. For this purpose he directs to keep the sore open for several weeks, and to promote the discharge by means of stimulant applications. When the part is bitten by a serpent, whose venom is known to be very mortal, such as the asp or *cerastes*, he advises immediate amputation. But his external remedies in general are calefacient and stimulant applications, such as liquid pitch, hot vinegar, sea-water, onions, nitre, salt, and mustard. He also directs to apply the warm body of a newly killed fowl, which being, he says, of a hot nature, absorbs, and as it were sucks the poison from the wound. He mentions many internal remedies, such as bitumen, birthwort, rue, dill, garlic, marjoram, fennel, &c. given in wine or otherwise. His intention in administering these medicines was evidently to restore animal heat and support the strength. As I do not mean to enter minutely upon the subject of theriacs, I shall not go into an exposition of his treatment of particular cases.

The subsequent writers on theriacs, namely, Galen, Aetius, Oribasius, Paulus Ægineta, Nonnus, and Actuarius are the professed copyists of Dioscorides, and therefore I shall only make a few remarks upon their methods of treatment, in order to show how his views were afterwards acknowledged and followed. Galen lays it down as a general rule of treatment, that the object of cure is either to effect the evacuation or alteration of the poison. The evacuation is to be accomplished by cupping-instruments, sucking the wound, cauteries or escharotics. The alteration is to be performed by medicines which excite a counter-action; as, for example, if the poison produces cold of the system, it is to be remedied by calefacients, or if heat by refrigerants.* There is little or no variation in the practice of the others. Paulus, as above-mentioned, allows the occasional use of general bleeding, but only after the poison has been already distributed over the body. The others do not mention venesection among their general remedies, but, on the contrary, direct the administration of strong stimulants, calefacients, and aromatics.

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* Meth. Med. xiii. c. 6. See also de Theriaca ad Pisonem et de Antidot. Lib. ii.

The Arabians have been condemned as the servile imitators of the Greeks; but was it not better to follow the beaten tract of knowledge, rather than to deviate into error? Neither in toxicology, nor in any department of medical science, do they profess to renounce the guidance of their Grecian masters; but they did what only remained for them to do, by applying the general principles formerly laid down to certain new instances which came under their experience. Accordingly, we find described, especially in their alexipharmics, the symptoms and treatment of several poisons, of which the Greeks appear to have been ignorant. Unfortunately, however, owing to the brevity of their descriptions, there is often no small difficulty in determining the substances of which they treat.

Avicenna lays it down as the general indications of cure, to support the animal heat so as to enable it to expel the poison, or to dissipate and evaporate the same. Like Paulus, he permits venesection in some instances when the poison has been distributed over the system; and makes mention also of purging, sudorifics, and emetics. He forbids the use of all external applications of an astringent nature, which he says prove highly pernicious, by restraining the discharge of the poison, whereas attractive or strongly stimulant medicines promote the evacuation of it. Like the Greeks, he approves of sucking the wound, of cupping, scarifications, leeches, burning, and in extreme cases, of amputation. His internal remedies are either hot aromatics, such as castor, birthwort, cinnamon, pepper, garlic, wine, and the like, or narcotics, in case there be inflammatory excitement, as for example, opium, mandragora, &c. The other Arabians do not treat so fully of those matters as Avicenna, but otherwise inculcate the same rules of treatment. Serapion, indeed, seems to direct, that the person who sucks a poisoned wound be fasting; but as he was a mere compiler, and lays no claim to originality, it is not likely that he should have ventured to oppose every preceding authority, and, therefore, I am inclined to believe that there must be an error in the text.

If the reader will take the trouble to compare this exposition, however imperfect, of the ancient principles of theriacs with the treatment recommended by late writers, he will find that the improvements introduced in modern times are neither numerous nor important. Of course, it will not now be pretended that the use of the cupping-glass and of the ligature, in cases of poisoned wounds, are new discoveries.

We shall now proceed to Alexipharmics, being the most important branch of toxicology.

Nicander treats of the following deleterious substances:—1. *Aconitum*, or wolf's-bane. 2. *Argenti spuma*, or litharge.

3. ***Buprestis***, probably a species of Scarabæus, resembling the *cantharis*. 4. *Cantharides*. 5. Ceruse, or white lead. 6. *Conium*, or hemlock. 7. Coriander. 8. *Dorycnium*, probably the *Solanum halicacabum*. 9. *Ephemeron*, or meadow saffron. 10. *Fungi*, or poisonous mushrooms. 11. *Hirudines*, or leeches. 12. *Hyoscyamus*, or henbane. 13. *Ixias*, probably a species of chamæleon.* 14. Coagulated milk. 15. The sea hare. 16. *Papaver*, or poppy. 17. *Pharicum*, probably a composition from agaric.† 18. The red toad and marsh frog. 19. The salamander. 20. Bulls' blood. 21. *Taxus*, or the yew-tree. 22. *Toxicum*, very doubtful.‡ He details the symptoms produced by swallowing these substances with great accuracy, but has not reduced his treatment to any fixed principles, which Dioscorides was the first to expound clearly; and, therefore, we shall take him as our guide here, and only refer to Nicander occasionally.

When a person is known to have swallowed some deleterious substance, the nature of which is not ascertained, Dioscorides directs to give hot oil in water, or separately, as an emetic. When oil is not at hand, butter melted in hot water, or a decoction of linseed, or of marsh mallows, may be given. These substances, he says, not only occasion the discharge of the poison, but also blunt its acrimony and at the same time open the belly. As a proof of the efficacy of oil in obviating the effects of acrid substances, he states, that if one will previously rub the skin with it, neither cantharides nor quicklime, nor any other such irritant, will produce ulceration thereof. It will also protect the body from the effects of astringents and refrigerants. After vomiting, the bowels are to be evacuated with a clyster, and nitre is to be given in hydromel, old wine liberally drunk, and fat flesh and fishes eaten in order to loose the belly, encourage further vomiting, and blunt the acrimony of the poison. He then enumerates various medicines as general remedies in such cases, namely, agaric, southernwood, castor, the long birthwort, &c.—all of which are to be taken in wine. He afterwards remarks how difficult it is to judge of the substance which has been swallowed from the symptoms, as various articles of this nature occasion nearly the same constitutional derangement.

The general directions given by Celsus for all cases of poisoning are brief: namely, vomiting to be excited by drinking much oil, taking an antidote, or, if that is not at hand, undiluted wine. The ingredients of his antidotes are mostly stimulants and aromatics, such as myrrh, pepper, spikenard, turpentine, rosin, iris, anise, &c. Of the subsequent authors, all of whom professedly copy from Dioscorides, Aetius may perhaps be instanced as the most copious and original. He directs the administration of emetics, which are to consist of oil, or of radishes, or of Lemnian earth, or of butter melted in hot water, or of nitre dissolved in water. When the poison has descended to the bowels, and raised pains about the navel, he recommends clysters containing mallows, fenugreek, centaury, &c. and directs to eat radishes and garlic, to drink wine prepared with salt water, and decoctions of diuretic herbs, and to use a bath of oil and water. It is when the poison has been determined to the skin that these baths are to be used. When the bladder is corroded, he recommends the seeds of cucumber and other things of a refrigerant nature. When there is a determination to the head, an emollient clyster is first to be injected, then sternutatories are to be applied, such as fullers' herb, pepper, the juice of elaterium; or aromatics, such as castor and the like.

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* The chamæleon appears to be the Carlina.

† See a learned note by Schneider in his edition of the Alexipharmics.

‡ Even Avicenna admits that he was unacquainted with the real nature of the ancient Toxicum.

As no additional information is to be derived from the other Greek and Latin authors, I proceed to the Arabians. Avicenna and Rhases direct to give immediately for an emetic oil of sesame or of olives, impregnated, if possible, with the virtues of dill, and afterwards to use demulcents, such as the decoction of linseed, melted butter and milk. The emetics are to be followed by clysters, if it appear that the mischief has spread downwards. If the disorder continue, another emetic is to be given, and the drinking of milk continued. When, after the emetic, inflammation supervenes, they direct to give snow water, or oil of roses, and to promote vomiting with them. They strenuously urge in all cases to prevent sleep, and to rouse by every possible means. If the poison is ascertained to be of a sharp penetrating nature, medicines are to be given to obtund its acrimony, such as milk, almond-oil, melted butter, &c. If it is ascertained to be of an inflammatory nature, the parts are to be cooled with camphor, rose-water, or the water of coriander; or all these things are to be congealed in snow, and applied over the vital organs. Blood-letting, if indicated, is to be practised. When the poison is known to be of a stupifying nature, the theriac and assafœtida in undiluted wine or garlic are to be given. When it is particularly deleterious, the cure is to be effected by means of the mithridate, theriac, musk, and other aromatics. The patient is to be covered with aromatic cloths, the pit of the stomach is to be rubbed, his face is to be fanned, his hair pulled, and in short, every thing is to be done to rouse him.

Avicenna makes mention of all the specific medicines recommended by Dioscorides.

Having thus briefly explained the general principles of ancient practice, we shall describe the application of them to a few particular cases. We begin with *Cantharides.*

The symptoms occasioned by swallowing cantharides, as described by Nicander, are erosion of the whole intestinal canal, ulceration of the bladder, affection of the chest, and wild delirium. His remedies are emetics, consisting of various fatty substances, clysters of milk, laxatives, and drinking milk and the decoction of vine shoots with honey. He also recommends a mixture of rue with oil of iris, probably for an emetic. This last remedy is mentioned by Galen, Dioscorides, and Scribonius Largus.

Celsus merely mentions panacea pounded with milk, galbanum with wine, and milk by itself, as remedies for cantharides.

According to Dioscorides, the symptoms of poisoning by cantharides are erosion from the mouth to the bladder, dysuria, bloody urine, alvine discharges like those in dysentery, nausea, *deliquum animi*, *vertigo*, and at last frenzy. To obviate these symptoms, he directs at the commencement an emetic of oil, which is to be followed by an emollient injection, for example the decoction of linseed or of mallows. Nitre [*] is then to be given in hydromel to clear out the bowels. If not thus evacuated, an injection of nitre dissolved in water is to be given, and then the patient is to take a vinous tincture of pine kernels, or the seeds of cucumber in milk, or mead, or else the grease of a goose melted in a weak sweet wine. The inflamed parts are to be anointed with a decoction of barley-meal in mead. Cataplasms, he says, prove prejudicial at the commencement, because by increasing the heat they promote the distribution of the poison; but after a time they are useful by soothing the pains. The patient is then to be put into a warm bath, in order to promote the discharge of the poison by the cutaneous perspiration. The bowels are to be frequently opened, and with this intention the food ought to consist of animal fats, which are at the same time laxative and obtund the acrimony of the poison. A sweet wine is to be liberally drunk; other remedies are mentioned, namely, the bark of the frankincense tree, Samian earth, pennyroyal, the ointments of iris and of roses, with the decoction of rue, and fresh vine shoots pounded in sweet wine. He particularly recommends antidotes given in honied water. We have already stated the nature of the ingredients which entered into the composition of the ancient antidotes, when treating of the general remedies.

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[*] It is scarcely necessary to remark, that the Latin *nitrum* and the Greek *νιτρον* was a very different substance from the modern nitrate of potass, but it is not so easy to determine its real nature. Geoffroy looked upon it to have been of the same nature as the salt of tartar or potash, but it is much more probable that it was a composition of soda. It appears from Martial (Lib. xiii, l. 14,) and Serapion, (de Simpl. ex Mineris,) that it was nearly allied to common salt, which we know to be a composition of soda. From the fact incidentally mentioned in the Bible, (Proverbs, xxv. 20,) that an effervescence was produced by pouring vinegar upon it, we may also determine with some confidence that it was a carbonate. It seems therefore likely that it was a carbonate of soda. Such is the opinion of the learned modern Greek, Dr Coray, no mean authority upon such a matter.—See his Edition of Xenocrates and Galen de Medic. ex Aquatilibus.

Galen singularly enough states that the wings and feet of cantharides prove a remedy for the deleterious effects occasioned by swallowing their bodies. I much doubt the efficacy of the remedy, but suppose that it was given with the intention of its acting as a diuretic.* His treatment otherwise, which is borrowed from Asclepiades, is very similar to that of Dioscorides.†

Aetius recommends emetics composed of fat animal soups, but says nothing of vegetable oils. Why he preferred those of animals I cannot determine. He also recommends milk, pine-kernels, the bath, and Galen's remedy mentioned above. Oribasius, Paulus Ægineta, and Actuarius, only copy Dioscorides, without suggesting any improvement. It is worthy of remark, however, that Paulus joins Dioscorides in condemning the use of hot cataplasms at the commencement, and approves of the hot bath only when the bowels have been cleared of the poison.

The Arabians treat of this case of poisoning somewhat more fully than the Greeks. Thus Avicenna and Rhases, in addition to the symptoms enumerated by Dioscorides, mention that cantharides occasion inflammation of the penis and erections. They recommend emetics with oil and water, or with a decoction of figs. Afterwards milk is to be given in large quantities to drink, or purslain water, or that of fleawort or the like, and clysters are to be administered consisting of the decoction of linseed, of mallows, or the like, with almond oil, the whites of eggs, fat broths, melted butter, honey, &c. Rhases approves of bleeding, provided it be long since the patient was bled, and violent pain is felt in the region of the thighs, that is to say, if the urinary organs are much affected. Both recommend opening the belly, and, what I believe is an improvement upon the practice of the Greeks, injections of rose-oil into the bladder by means of a waxen tube, *(factum ex cerâ pennam perforatam interius habens.)* They also mention oil of quince and the theriac.

It appears from the works of Ambrose Paré, and the commentaries of Matthiolus upon Dioscorides, that the treatment in their days was conducted exactly in the manner we have described it to have been managed from Nicander down to Actuarius. Even Orfila's plan of treatment is almost exactly the same; he recommends, particularly in the first moments, gentle emetics, such as oil in large quantities. It is remarkable that, with respect to this remedy, Dr Christison is at variance with all the great authorities we have mentioned.* He says, "the treatment of poisoning with cantharides is not well established. No antidote has yet been discovered. At one time fixed oil was believed to be an excellent remedy. But the experiments of Robiquet on the active principle of the poison, and those of Orfila on the effects of its oleaginous solution, rather prove that oil is the reverse of an antidote. The case mentioned in the Genoa memoirs was evidently exasperated by the use of oil."—P. 459.

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* De Med. Simpl. Lib. xi.

† De Antid. Lib. ii.

Not having read the detail of the case to which he alludes, I cannot pretend to account for the failure of oil to afford relief in that instance, but surely the authority of one case ought not to be allowed to overbalance the accumulated experience of two thousand years. Ready as I am to admit the fallaciousness of experience which the father of medicine has proclaimed in a memorable aphorism, I am loath to believe that a pernicious mode of practice could have been followed for so many ages without being remarked. Dr Christison approves of oleaginous and demulcent injections into the bladder, first proposed, as we have mentioned, by the Arabians. He also mentions in general terms the warm bath, venesection, and bleeding by leeches, according as the degree and stage of the inflammation may seem to indicate. Upon this part of the treatment I would beg leave respectfully to offer a few remarks. Is there any foundation for the apprehension entertained by Dioscorides and Paul of Ægina, that cataplasms and the warm bath will prove prejudicial if used before the poison has been removed from the intestines? If the hot bath open the pores of the skin, and occasion a determination outwardly, will it not produce a corresponding emptiness of the deep-seated vessels? And agreeably to a principle of the animal economy already stated, as having been acknowledged by the ancients, and lately pointed out again by M. Magendie, will it not thereby accelerate the absorption of the poison? The same objection applies still more strongly to venesection, lest by emptying the vessels it should tend to distribute the poison over the system. This is a rule of practice, the application of which, in a variety of cases, I could wish to see more clearly laid down than it is at present; for I have often observed that practitioners feel no reluctance in having recourse to venesection at a time when they are aware that the stomach and bowels are loaded with vitiated secretions, and all sorts of deleterious *ingesta*, regardless of the injunctions delivered by the ancient authors,* who forbade evacuation by bleeding until such matters had been removed from the intestines. I have already referred to the sentiments of Dr Paris upon this point, which are so much to my mind, that I shall give them fully in his own words, although they are applied by him to a different case of poisoning from that of which we are now treating. "The reader is requested to refer to our *exposé* of the celebrated doctrine of Magendie, and he will see that *venesection* proves one of the most powerful means of exciting the function of absorption; hence in poisoning by arsenic, such an expedient should never be recommended while a particle of that substance remains in the body." He adds in a note, † "Notwithstanding this fact, we find venesection recommended in works on toxicology as a mere precaution to be used against the inflammatory action produced by arsenic." ‡

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* The learned author appears here to have overlooked the opinion expressed by *Professor Orfila* in the last edition of his *Toxicologie*, (1826.) "L'huile d'olives, prônée pendant long-temps comme contrepoison des cantharides, a été rejetée avec raison par *M. Pallas*, parcequ'elle augmente les accidens. On sait en effet qu'elle dissout le principe actif des cantharides, et nous avons vu qu'il suffisait des quelques heures pour faire périr les animaux auquels on avait administré de l'huile ayant agi pendant quelque temps sur la poudre de cantharides." (ii. 36.)—EDITORS.

We shall now give some account of the ancient treatment in cases of poisoning by *opium*. Nicander's description of the symptoms, when his language is stripped of poetical embellishment, is as follows. Coldness of the extremities, heaviness of the eyelids, profuse perspiration, paleness of the skin, swelling of the lips, relaxation of the under jaw, the respiration slow, the breath cold, and the usual precursors of dissolution, distortion of the nostrils, lividity of the nails, and hollow eyes. He recommends to produce vomiting by introducing oil of roses or of iris into the throat upon wool. The oil of iris was reckoned peculiarly adapted for such cases, because its properties are both stimulant and emetic. He then directs to give hot wine and honey, either to counteract the narcotic effects of the poison, or perhaps to encourage further vomiting. (See Scribonius Largus.) The patient is to be roused by cries, by striking his body in different places, and by wrapping it in cloths smeared with oil, and hot wine. Hot drink, and the hot bath are to be used with the intention of resisting the tendency of the blood to thicken, and of relaxing the skin contracted at last by the cold nature of the medicine.

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* See in particular Averrhoes Collectan, § iii. Colliget, lib. vii. c. 1.

† Albucasis Chirurg., lib. ii. c. 97.

‡ Pharmacology, Vol. I. p. 255.

Dioscorides's description is very short and imperfect. The only symptoms he mentions are lethargy, violent itching, and the perspiration smelling of opium. His treatment consists of emetics of oil administered at the commencement, then acrid clysters are to be given, and afterwards oxymel with salt is to be drunk, or honey with hot rose-oil, or wine with southernwood or cinnamon, or hot vinegar by itself, or nitre dissolved in water, and marjoram with lye or sweet wine, or the seed of wild rue is to be given with pepper, wine, and all-heal, or pepper with castor and hydromel, or with the decoction of savoury, or marjoram with wine. The patient is to be roused with strong-scented things; and to relieve the pruritus, baths and fomentations are to be used. After the bath, fat broths with wine are to be given.

Galen states that opium dissolved in a small quantity of wine produces stronger effects than when given alone, but that a greater quantity of wine proves an antidote by counteracting its refrigerant or narcotic powers. He mentions that he had once cured a person reduced to the last stage of coldness, by administering a strong, light-coloured, and fragrant wine. He attributes the pernicious effects of a small quantity to its accelerating the distribution of the opium over the system, for, he adds, when this takes place slowly, there is a chance that the poison may become altered or digested. * He conceives that opium acts upon the heart, occasioning coldness thereof. †

Aetius gives nearly the same detail of symptoms as Nicander. He adds that convulsions come on at last. He recommends exactly the same treatment as Dioscorides, with the addition of the theriac. None of the other Greek writers make mention of convulsions among the symptoms.—They are, in short, mere copyists from Dioscorides.

The Latin author Scribonius Largus gives a brief but accurate account of the symptoms and treatment. He directs, after repeated vomiting, to apply to the head embrocations of vinegar and roses, to use frictions of the feet, and to put sinapisms to the feet and thighs.

Of the Arabians, Avicenna and Rhases enumerate pruritus among the symptoms, and also mention that convulsions come on before death. They concur in recommending repeated vomits with oil, or water and salts, or with hydromel, dill and

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* Dr Christison says in like manner, "Vegetable and animal poisons may be altogether destroyed by the process of digestion." P. 52.

† De Med. Simp. I.. iii.

Indian salt. Acrid clysters are then to be administered. Afterwards various calefacient and aromatic substances are to be given, such as wine with pepper, cinnamon, southernwood, castor and rue, along with vinegar. They particularly recommend a composition consisting of equal quantities of assafœtida, castor, pepper, and juniper berries, (Rhases,) or savin, (Avicenna.) Avicenna greatly praises the solution of one drachm of assafœtida in twenty-five drachms of fragrant wine. Both mention the theriac. They direct to pull the patient's hair, to prevent sleep, to give sternutatories, to apply friction with calefacient oils, and to use hot baths, in order to relieve the pruritus, and prevent convulsions. Avicenna says, that opium acts by occasioning a thickening of the blood, impairing the animal heat, and acting upon the respiratory organs.

According to Serapion, Rhases, Avicenna, and, I believe, Hali Abbas, the smallest dose of opium which will prove destructive of human life is two drachms. Modern authors are not agreed as to the smallest quantity which may prove fatal, but surely, as Dr Christison remarks, Dr Paris has fixed the minimum dose too low, when he affirms that four grains may be sufficient to produce this effect. On the other hand, I should think that a smaller dose than that mentioned by the Arabians might be sufficient to destroy life.

It will be remarked that Aetius among the Greek, and Rhases and Avicenna among the Arabic authorities, enumerate pruritus among the symptoms of poisoning with opium, but I do not find this symptom mentioned in the reports of the cases given by Orfila and Christison. Both, indeed, state that itching of the skin is a symptom of the operation of morphia on the constitution, but do not say that this effect is produced by the other preparations of opium. Dr Paris, however, has mentioned this effect of opium. "The operation of opium is not unfrequently attended with an itching, or sense of pricking of the skin, which is sometimes terminated by a species of miliary eruption." *

It will also be remarked, that convulsions are mentioned by some of the ancient authorities, and remitted by others. Although mentioned by Dr Paris, Dr Duncan (Edinburgh Dispensatory,) and most modern authorities on toxicology, it appears from Dr Christison's account, that they are an occasional but not a frequent effect of opium.

The symptoms, as detailed by Nicander, will be found, upon reference to the cases collected by Orfila and Christison, to be

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* Pharmacology, Vol. ii. p. 355.

upon the whole exceedingly correct. In the original his description is singularly graphic.

The comparison of the ancient and modern views of practice may lead to some interesting reflections. It will be perceived that most of the ancient authorities recommend vinegar in cases of poisoning by opium, but none, I am inclined to think, gave it at the commencement, nor until the poison had been removed from the bowels. This rule agrees very well with that laid down by Orfila, Paris, and Christison, who say that vinegar is prejudicial, if given at first, by favouring the solution of the poison, but useful afterwards as a restorative of the system. Vinegar and pepper formed the common remedy used by the ancients in cases of syncope, and suspended animation, to which the condition of the body in poisoning with opium bears some resemblance. The ancients undoubtedly were aware that vinegar does not diminish the sedative property of opium, for Serapion, who is a mere retailer of established opinions, recommends opium dissolved in vinegar to allay the pain of acute abscesses. Galen, as we have mentioned above, was aware that a small quantity of wine increases the deleterious effects of opium. He states that this is particularly the case with a weak wine, "which is just sufficient to break down the poison, and assist in sending it to the heart in the current of distribution." * Now, as a weak wine may be supposed to act very like vinegar, we are warranted, I think, to infer from this language, that Galen's views were not very dissimilar to those of Orfila.

Venesection is not mentioned by any of the Greek, Latin, or Arabic authorities, † but is a method of treatment altogether modern, and one, regarding the application of which considerable difference of opinion prevails. Thus, Dr Christison says, that in certain cases it has seemed injurious; and he states farther, that venesection will be unnecessary when the stomach has been emptied. Of course, then, he means to inculcate, that if ever venesection prove useful at all, it must be while the

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[*] De Med. Simp. Lib. iii.

[†] Although none of the ancient authorities on toxicology, as far as I know, has sanctioned the use of venesection in cases of poisoning with opium, Alexander of Tralles, a very acute and original writer, permits depletion in a parallel case, namely, when the powers of the system are oppressed by a redundance of crude humours in fevers. If, says he, the load be so great that there is danger lest the powers of the system be choaked, it will not be improper to diminish the quantity and lighten the burden. We see this happen, he adds, with a fire. For when it is in danger of being extinguished by green fuel, it often happens, that, if a few pieces are removed, it breaks forth and overcomes the load that pressed upon it. I conceive that Alexander has here stated correctly the principle upon which venesection may sometimes be serviceable in cases of congestive fever, and of poisoning with narcotics.—Lib. xii. c. 3.

poison remains in the stomach.[*] Dr Paris, on the other hand, lays it down as a rule, " that venesection should be performed with extreme caution and judgment after a narcotic poison;" and, as we have already mentioned, in all cases of poisoning he forbids venesection until the stomach has been cleared of the deleterious substance. It appears, therefore, that his views and those of Dr Christison do not very well agree. Although Orfila's experiments, and some cases which I have seen reported, seem to bear in favour of venesection, I am inclined to think, that at best it must be looked upon as a *remedium anceps.*

The assafœtida, castor, and other medicines of the same class, recommended by the ancients, seem to have been given with decided advantage in Dr Marcet's case, related in the first volume of the Medico-Chirurgical Transactions.

The ancients, it will be seen, used the hot bath in cases of poisoning with opium. Its efficacy as a restorative after the removal of the lethargic symptoms will scarcely be questioned; but, even whilst they are are present, a plunge into the hot bath might tend to rouse the system. If I recollect right, Dr Currie recommends the affusion of hot water in such cases. Of late the cold affusion has been often used, but may prove dangerous, as Dr Christison remarks, when the heat is diminished considerably.

In order to understand properly the ideas entertained by the ancients respecting the *modus operandi* of opium, it will be necessary to say a few words in explanation of their opinions upon certain points of physiology. Aristotle[†] taught that the prime cause of all the operations of life is mind, and that the prime instrument by which it performs them is heat, which, therefore, he denominates the *co-cause* (συναιτιον.) He illustrates his meaning by comparing mind to the artificer, and heat to the wimble or saw by which he performs his work. Having remarked, no doubt, that the heart was the warmest part of the body,[‡] he appears to have considered it as the spring which turned the whole machinery of the animal frame, the brain and nerves deriving their origin and influence from it. I need scarcely mention how well these ideas accord with the ingenious hypothesis lately advanced by M. Serres. Many facts, indeed, seem to point out the supreme importance of the heart. It is, as the ancients remarked, the *primum movens et ultimum moriens;* and, along with its accessary organs, the lungs,[*] it is evidently the part which, in the higher classes of animals, renders them independent of the many variations of temperature to which they are subjected. It is this wonderful organ which, under the guidance of the principle of life, preserves the heat of the body unaltered in all the different gradations of temperature, from more than 100 degrees above the boiling to as many below the freezing point of the thermometer. It seems, in fact, a real Prometheus that steals the fire from Heaven. The connection between heat and the vital actions is very apparent also in the inferior animals, who are not provided with such an apparatus for preserving equability of temperature. Thus the Zoophyta, Insectæ, and Vermes, with the loss of heat, lose also sensibility and muscular energy, and recover them again when their heat is restored. In this case it is evident that heat is the cause (or at least the *co-cause*) of the vital actions, and not the vital actions of heat. It has always appeared to me a striking fact, illustrative of the great influence of heat over the vital actions, that the strength of all animals is, bulk to bulk, proportionate to the degree of their animal heat.[†]

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[*] This is not altogether a correct interpretation of *Dr Christison's* observations. He says, " If the stomach be emptied, *and the patient kept roused*, venesection will be unnecessary. Sometimes, however," (that is in the preceding circumstances) " when the pulse is full and strong, it may be prudent to withdraw blood, &c.," and he distinctly states at the commencement of the section, that " the primary object is to remove the poison." His observations on the employment of blood-letting may be too succinct; but the natural interpretation is, that when the stomach has been emptied, *and the patient cannot be kept roused*, venesection may be useful; and in fact it is in these circumstances that its good effects have been most apparent.—EDITORS.

[†] See De Partibus Animalium, Lib. ii. c. 7.—De Animâ, Lib. ii. c. 4.—Historia Animalium, Lib. iii —De Respiratione,—De Spiritu. On the connection between Heat and Vitality, see an admirable passage in Cicero, De Nat. Deor. Lib. ii. § 9.

[‡] This fact is now, I believe, generally admitted.—See Dr Davy, Philos. Tran-

This doctrine of the supreme authority of the heart, as being the focus of heat, thus maintained by Aristotle, was eagerly defended by his great Arabian commentator,[‡] Averrhoes, and by his countryman, Avenzoar, who keenly attacked Galen for having questioned its truth, and taught, as they represent, that the brain is the leading principle in the animal frame. After having, however, carefully ransacked every part of Galen's

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sact. 1814. I need scarcely mention, that all the ancient writers on anatomy agreed with Aristotle in reckoning the heart the focus and fountain of animal heat, or, as they called it, innate heat. See Ruffus de partibus Hominis, Galen de Usu partium, Lib. vi., Orebasius Anatomica, Theophilus de Fabrice Hominis, Lib. iii.

[*] The ancients looked upon the lungs as being entirely subservient to the purposes of the heart. Aretæus says, "The heart is the source of life and respiration; for it imparts to the lungs the desire of drawing in cool air." De Morb. Acut.—Theophilus says in like manner, "The respiratory organs are formed on account of the heart, that the innate heat in it may be cooled, increased, and nourished." De Fabr. Hominis, Lib. iii. That respiration is, in certain cases, a cooling process, is admitted by Dr Crawford himself.—On Animal Heat.

[†] Birds are the warmest and strongest of all classes of animals. M. L'Abbé Bonnaterre says of them, "La Nature a resemblé et concentré dans le petit volume de leur corps plus de force qu'elle n'en a departi aux quadrupedes les plus puissans."—Encyclop. Method. xi. Livraison.

[‡] Averrhois che 'l gran commento feo.—Dante Inf. Canto 4.

works[*] in which I could suppose it likely to meet with any allusion to this doctrine, I am led to believe that these Arabians, in the heat of controversy, have misrepresented the real opinion of their master's rival. Galen appears decidedly to have maintained with Hippocrates,[†] "that there is in the body no one beginning, but that all parts are alike beginning and end. For a circle has no beginning." Agreeably to this idea, Galen remarks, that the brain cannot properly be said to derive its powers from the heart, since an animal will run, breathe, and cry after its heart has been taken out; nor can the heart be said properly to derive its powers from the brain, since it will palpitate and contract after all intercourse with the brain is cut off, nay, after it has been removed from the body.[‡] In so far, then, the functions of the brain and the heart are independent of one another. But the brain is dependent upon the heart and its appendages for animal heat, without which it would be unable to continue its functions; and the heart, on the other hand, is dependent upon the brain for imparting nervous influence to the respiratory organs, without which the heart could not preserve its vital heat unaltered. Hence the mutual connection and sympathy of important organs,—a doctrine much insisted upon by ancient authors, and which bears some resemblance to the theory lately advanced by Mr Morgan and Dr Addison.

We shall now have no difficulty in understanding the ideas of the ancients regarding the operation of opium. Galen and Avicenna believed, as we have stated, that the poison exerted its primary influence upon the heart, and impaired its vital heat. Of course they considered its operation on the brain as secondary. They called the action of narcotics frigorific or congealing,[§] no doubt because they remarked that it was attended with a diminution of animal heat; and to this they attributed the loss of sensibility and muscular energy. The resemblance between the state of profound sopor and diminished sensibility, produced by exposure to extreme cold, and the symptoms occasioned by an overdose of a strong narcotic such as opium, must be admitted to be a striking fact in confirmation of the ancient views. I leave it to the reader to judge whether the theory or the modification of it proposed by Messrs Morgan and Addison be the more plausible. I shall merely observe, that the symptoms of the cases related by Dr Christison and M. Orfila accord very well with the ancient explanation of the action of opium. Coldness of the body, particularly of the extremities, slow respiration, pulse generally slow, and always feeble, are just such effects as we might expect from the functions of the heart being disordered, and the transmission of animal heat interrupted. The appearance of the blood, which in almost every case is described as being black and thick, may probably be referred to the same cause. This theory likewise furnishes a satisfactory explanation of the fact, that none of the poison can usually be detected in the organs which appear to be most affected, their deranged actions having been occasioned not by the presence of any deleterious substance, but by the want of the natural stimulus, supplied by what the ancients called animal spirits, or what in the chemical nomenclature are denominated oxygen gas and caloric. But I will not enlarge farther upon this subject at present. Enough, I trust, has been said to convince any unprejudiced person that much useful information may be derived from the study of the ancient writers on toxicology.

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[*] I have examined particularly De Usu partium, Lib. vi. and vii. De Administrationibus Anatomicis, Lib. vii. De Placitis Hippocratis et Platonis, Lib. i. and ii. and De Usu Respirationis.

[†] De locis homine. This ancient doctrine is well stated and defended in Abraham Kaau's incomparable work, entitled, Perspiratio dicta Hippocrate.

[‡] Avicenna states, that the heart will contract more than an hour after it is removed from the body. Nysten found that the strong stimulus of galvanism would sometimes make the auricles contract four hours and forty minutes after death occasioned by decapitation. See Researches de Physiol.

[§] The Athenian mode of performing public executions of condemned persons, by administering to them a draught composed of conium or hemlock, would make the ancients familiar with the effects of that narcotic on the human frame. The symptoms in the case of Socrates are well described by Plato. First, he felt a torpor of the limbs, then his feet lost their heat and sensibility; afterwards the legs and thighs became cold; then the lower part of the belly also became cold and stiff; and when the cold reached the heart he expired.—Phædo.

The conium is a better specimen of a narcotic medicine than opium, which appears in certain circumstances to act as a stimulant.

ART. XI.—*Dr Williams's Reply to Criticisms in the Medico-Chirurgical Review.* *

IN the Fasciculus of the Medico-Chirurgical Review for this month, are the following strictures upon my essay—On the

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* *Note by the Editors.*—On the subject of Dr Williams's paper we have received several communications from different correspondents. To insert these would occupy much more of our pages than in our estimation would be proper; and all of them, we trust, will be satisfied with the general assurance, that, however desirable it may be to criticise or defend the speculations of Dr Williams, we cannot allow that this Journal is a suitable field for such exercise. One only of these papers, that from Dr Williams himself, partly in defence, partly in further explanation of his views, we admit for the present, chiefly because the original paper appeared in our pages, and because the author conceives himself entitled to advance, in defence of his opinions, certain experiments which seem calculated to that purpose. We must, however, say, that after this we can admit no further papers on the subject, since it appears to assume a controversial aspect; and it need to be no matter

Sounds produced by the Action of the Heart,—which you did me the favour of inserting in the last Number of your Journal. The reviewer, after quoting that part of my essay in which I explain the cause I conceive to produce the second of the two immediately successive sounds heard in exploring the action of the heart, proceeds thus:—" The foregoing theory is very laboured, very ingenious, and very incorrect. It is a pity that when Dr Williams was reviewing the anatomical construction of the heart with such scrutinizing eye, he did not look at those auriculo-ventricular valves whose function he mistakes. Dr Williams argues boldly that the musculi papillares or columnæ carneæ open the valves; it is rather unfortunate for him that they *close* it. Take, for instance, the mitral valve:—its two alæ are not supplied by separate packets of musculi papillares, which, if so arranged, might by possibility pull those two alæ asunder, and open the valve; no! one batch of the columnæ carneæ sends off chordæ tendineæ, which diverge and supply *the opposite edges of either ala*, thus obviously and beautifully operating to approximate them and thereby close the valve. This is a plain anatomical fact, which Dr Williams ought to have known, and which, if known, would have saved him the trouble of writing more than eight pages of close letter-press to prove his error. The mistake is so palpable, that, had it not been made by a writer of Dr Williams's celebrity, we should never have thought it worth correction. As, however, his authority might lead some astray, we have thought it our duty to strangle the misconception in embryo, lest it should grow into a mischievous maturity. It is needless to observe, that the foundation being thus withdrawn from beneath its feet, the whole series of speculations falls at once into the abyss of nothingness. We have only, in conclusion, to express our belief, that the second sound heard on exploring the heart is, what Laennec unhesitatingly asserted it to be, that of the auricular systole, and that this belief has not yet been shaken by the arguments of Mr Turner or the theories of Dr Williams."

It is evident, from the tenor of these remarks, that the reviewer imagines the auriculo-ventricular valves to be closed by the *contraction* of the musculi papillares. From the anatomical construction of the auriculo-ventricular valves now before me, I am inclined to think that this conviction of the reviewer is founded rather on the " many unanswerable reasons for the musculi papillares closing the valve," than on the arrangement of the chordæ tendineæ, or he would not have used the positive language his criticisms are expressed in. Impressed with this idea, I shall feel obliged if he favours me with those experiments and observations from which his conclusions are deduced. In the meantime, I beg to call the attention of my readers to experiments which I myself

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of astonishment, if we declare at the same time, our intention to give no opinion whatever on the merits of the question at issue, or the conclusiveness of the arguments adduced.

consider to argue strongly that the *contraction* of the musculi papillares is not absolutely necessary for the adjustment of the alæ of the auriculo-ventricular valve when in the act of performing the valvular office; and further, that their *contraction* is not absolutely necessary even to prevent the margins of the valve being carried back into the auricle when the sides of the cavity are brought into contact.

1*st Exp.*—Having removed the auricles of a sheep's heart, and retained about an inch of the origins of both the aorta and pulmonary artery, the semilunar valves were cut away, and the brass end of one of Weiss's elastic tubes was secured in the pulmonary artery. After this, by means of his patent syringe, I injected into the ventricle water, which, as it filled the cavity, raised the valve, and when the ventricle became fully distended, no water escaped between the alæ of the valve. By raising the handle of the syringe, the water was withdrawn and the valve opened; thus by pressing down and raising up the piston the phenomena of the action of the valve during life was in some measure imitated. The depression of the piston represents the mechanical effect of the systole of the ventricle in closing the valve; and the pulling of the piston, as it appears to me, that of the muscular action of the musculi papillares in opening the valve.

2*d Exp.*—An elastic tube being fixed in the aorta, the mitral valve underwent the same trial as the tricuspid, and with the same result. To ascertain whether the margins of the valve could be pressed back into the auricle, by approaching the parietes of the ventricle towards each other by external pressure, the right ventricle was removed to allow the left to be more readily grasped. When the latter was fully distended, it was embraced by both hands, and the pressure was carried so far as to force the greater part of the water through the parietes of the ventricle; yet the margins of the valve were not carried into the auricle, nor did any water escape through the valve while fully expanded.

I need not contend that there was no muscular contraction to adjust the alæ of the valves in these experiments. If I may presume, however, to suppose the effectual closing of the auriculo-ventricular valve during life, to be effected on the same principle as it was accomplished in the above experiments, may I ask whether the contraction of the musculi papillares simultaneously with the systole of the ventricle would not interfere with the office of the valve? In my experiments, apparently the greater the pressure applied to the ventricular surface of the valve, the more perfect the closure. From this circumstance may it not be inferred, that the contraction of the musculi papillares does not take place until the valve has performed its office of flood-gate, in preventing the retrograde motion of the blood during the systole of the ventricle. For if the valve closes more perfectly as the pressure on its ventricular surface is increased, the simultaneous contraction of the musculi papillares with the systole of the ventricle could only have the tendency of making the valves less effective, by acting as an antagonist to the impulse or pressure which is necessary to the closing of them effectually.

I have received the following note from Dr Traill. My friend's opinion inspires me with no little confidence, that I have not made the palpable mistake my reviewer conceives; and I do not doubt but that the observations of Dr Traill will be justly appreciated by my readers.

" *St Anne Street, November* 8, 1829.—MY DEAR SIR,—I have witnessed your experiments on the effect of injecting fluids into the ventricles of the heart of different mammiferous animals with much satisfaction. In these experiments, the uniform result of filling the ventricles from the pulmonary artery and aorta is effectually and firmly to *close* the auriculo-ventricular valves, while the withdrawing the liquid by the syringe immediately *opens* them; and it appears to me, both from a consideration of the direction of the fibres of the musculi-papillares, and from your experiments, that the consequence of a *contraction* of these muscles during life must be to *open* the tricuspid and mitral valves; while their relaxation and the pressure applied to their ventricular surfaces by the systole of the ventricles must be to *shut* these admirably contrived flood-gates.—I am truly yours,

THOMAS STEWART TRAILL.

Liverpool, November 9th, 1829."

PART II.

CRITICAL ANALYSIS.

ART. I.—1. *A Treatise on Fever.* By SOUTHWOOD SMITH, M. D., Physician to the London Fever Hospital. Longman, &c. London, 1830. 8vo. Pp. 436.

2. *Clinical Illustrations of Fever, comprising a Report of the Cases treated at the London Fever Hospital in* 1828–9. By ALEXANDER TWEEDIE, M. D., Member of the Royal College of Physicians, London; Physician to the London Fever Hospital, &c. Whitaker, &c. London, 1830. 8vo. Pp. 204.

3. *Notions of the Nature of Fever and of Nervous Action.* By WILLIAM FORRESTER BOW, M. D., Senior Physician to the Alnwick Dispensary, and formerly Assistant-surgeon of his Majesty's 27th and 77th Regiments. Longman, &c. London, 1829. Pp. 100. 8vo.

WE agree entirely with the sentiments which have been expressed by *Dr Tweedie*, and which appear to have been judiciously enforced on the attention of himself and his colleague *Dr Smith* by the governors of the institution under their care,—that " the public have an undoubted right to expect that the information acquired in hospitals should not be confined to those to whom the care of the sick is entrusted, but that by impartially and faithfully recording the symptoms, causes, and treatment of disease, together with morbid appearances observed on dissection of those who die, these charities should not only be a blessing to the sick, but by thus diffusing the most valuable kind of knowledge, prove a source of universal good." In return, the medical officers of such institutions have an undoubted right to expect from the public, and those who direct public opinion, that, since the obligation of authorship is thus in some measure imposed on them, their efforts shall be received with a favourable eye, and without extravagant expectations of perpetual novelty and originality. Without this mutual understanding, the hospitals of Britain will soon become again, what they lately were, storehouses of information, known only to a few, and perishing with them.

Proceeding with these impressions to give an analysis of the works now before us, we shall commence with the Treatise on Fever by *Dr Smith*, as being the greatest of the three in extent, and also the largest in its pretensions.

Dr Smith does not confine himself to a detail and generalization of the facts which have presented themselves to his own notice, in other words, to a narrative of the Continued Fever of London, as he has witnessed it during the last few years, but undertakes to describe fever in general, and to rectify some fundamental errors at present entertained in various quarters respecting the pathology and treatment of the whole genus. Having discovered, as he thinks, that "perhaps there is no disease so little understood as the ordinary fever of this country, and none by the mismanagement of which so much life is lost," he resolved to free himself of slavish deference to authority and preconceived opinions, and to interrogate nature alone with the view of discovering in what the real essence of fever consists. By these means, and by pursuing his investigations progressively from particular facts to principles more and more general, he has arrived at conclusions materially different, he conceives, from those of previous authors, and such as he clearly looks upon as original discoveries. Let us see in what consist his claims to this merit,—a merit which would be of no ordinary kind, considering the subject to which his supposed discoveries relate.

In an introductory chapter, after sketching out the ideas of fever entertained by Hippocrates, Galen, Sydenham, Cullen, and Brown, he dwells more fully on the modern doctrines of Broussais and Clutterbuck on the one hand, and of Stoker, Burne, and a host of late writers on the other. Broussais and Clutterbuck, it is hardly necessary to remind the reader, insist that there is no such thing in nature as an essential fever, and that every fever depends on some local inflammation, which Broussais believes constantly to reside in the gastro-intestinal mucous membrane, and Clutterbuck imagines to have its seat as constantly in the brain. Drs Stoker, Burne, and most British physicians, on the contrary, defend the doctrine of essential fever, consider the local inflammations as accessory circumstances merely, and suppose that continued fever is essentially and from the first connected with a peculiar depressed state of the nervous system, which greatly modifies all the accidental phlegmasiæ occurring in the course of the disease, and which is very well known to the medical reader by the terms Asthenia, Adynamia, or adynamic state of the constitution. Now *Dr Smith* conceives that both of these sects of physicians are wrong. He espouses, on the one hand, the doctrine of the essential nature of continued fever,—maintaining that the febrile state is the fundamental and primary affection. But he adds, that this febrile state necessarily and in every case induces ere long some local inflammation, as it cannot exist for a moderate length of time without finding some organ or other in the body of each patient which is predisposed to undergo the inflammatory process. In short, he reverses the doctrine of the Broussaïsts. Broussais thinks continued fever always originates in local inflammation; *Dr Smith* that continued fever always produces local inflammation. On the other hand, he attacks the principles of the *Adynamic* physicians, insisting that a great deal too much influence has been assigned to adynamia or depressed nervous power as an essential and primary part of fever; insinuating, in fact, that adynamia is little else than a bugbear altogether, that it is a secondary affection, and merely indicates great violence or protracted duration of the early stage of the disease or stage of excitement.

From a careful generalization of his facts, he farther establishes in more specific terms that continued fever consists in three successive morbid states, which follow one another in an invariable order,—derangement of the sensorial and nervous powers, derangement in the circulating function, and derangement in the secreting and excreting functions. This is his definition of fever. Wherever these derangements occur in this order, the disease is fever; and fever cannot exist without each being present in the order assigned. The local inflammations which he represents to occur invariably in one organ or other, but nevertheless to be secondary, take place during the deranged state of the circulation, and in consequence of that derangement.

The species or genera of fever which he thinks ought to be admitted in conformity with the preceding definition are much fewer in number than nosologists usually admit. He denies the existence of the *Synocha* of Dr Cullen altogether, maintains that his *Typhus gravior* is at all events extinct now, whatever may be thought of the reality of its former existence, and believes that all fevers are essentially varieties in degree of the same nosologist's *Synochus*. In conformity, however, with custom, he divides fever into two genera, and each genus into two species, under the designations of *Synochus mitior*, *Synochus gravior*, *Typhus mitior*, and *Typhus gravior;* but he considers that these all differ from one another in severity merely.

The preceding paragraphs contain in a few words the leading general principles which *Dr Smith* desires to inculcate as to the nature and pathology of continued fever.

On reviewing them it appears to us in the first instance, that there is not, as he seems to suppose, any particular novelty in his definition of the disease. It is very generally acknowledged, we apprehend, by those who maintain the essential nature of fever, that it consists of derangement of the brain, derangement of the circulation, and derangement of the secretions and excretions, occurring in the order here enumerated. It is probably a new idea, indeed, that this description may be converted into a definition. But doubts may be entertained whether much is gained to nosography by Dr Smith's supposed improvement; for it certainly appears to us that nothing could be more general, more vague, we may almost say, than his projected definition, or less likely to communicate any fixed idea to the learner. And, besides, it is not correct to say, although Dr Smith repeatedly asserts so in somewhat dogmatical language, that the definition is not liable to exception; for the effects of concussion of the brain, and the phenomena of softening or inflammation of the cerebral substance will come under his definition perfectly.

Secondly, There appears no just reason whatever for rejecting Cullen's *Synocha* from among the genera and species of fever. It may be quite true, that no such disease has presented itself to Dr Smith's notice during the few years he has attended to the subject at the Fever Hospital of London; and in all probability it has been of late scarcely ever observed in the metropolis, since *Dr Tweedie* too does not mention it, and *Dr Burne* in his work lately analyzed in this Journal doubts its existence. But if Dr Smith will turn to our review of *Dr Burne's* treatise he will find a true Synocha described in the most explicit terms possible, as occurring not infrequently both in this city and in various Irish epidemics. (xxx. 413.); and he will also find it distinctly noticed in *Pinel's Nosographie*, as well as by various writers who have described the British Epidemic of 1816–20. We are surprised our author should exclude synocha from his species of fever, because it is really the best example he could find of fever without material primary adynamia,—of fever, where the adynamic state depends in a great measure on the violence of the disorder in the circulation.

Thirdly, As to the existence of Cullen's *Typhus gravior*, *Dr Smith* is undoubtedly correct in believing it is now almost or entirely extinct. We have seen it, however, twice, perhaps thrice, out of some thousand cases during the last twelve years. In these the worst characters of Cullen's *Typhus gravior* were present, namely, collapsed features, black retracted tongue, purple petechiæ, vibices, tendency to gangrene, almost imperceptible pulse, all occurring on the second or third day of fever, and without any precursory excitement so far as could be ascertained.

Fourthly, Dr Smith is, in our opinion, as clearly incorrect in throwing doubts over the existence of adynamia, or depressed nervous power (independent of precursory febrile excitement) as in denying the existence of synocha. We cannot imagine that any person of discernment could personally superintend in an hospital the progress of several epidemics such as have prevailed in the Infirmary of this place during the last twelve years, without being convinced, that there occurs in fever a depressed state of the nervous power which cannot perhaps be better described than by the ideas now attached by nosologists and physicians to the term adynamia; and that this adynamia varies in degree in different epidemics, so that in one epidemic the leading variety of disease is an inflammatory fever with little nervous depression, and with frequent secondary local inflammations, and in another the leading variety corresponds with all the characters of the *fiévre adynamique* of *Pinel.* We are quite ready to agree with *Dr Smith*, that the operation of an adynamic state of the system has been allowed too extensive an influence on the opinions and practice of some physicians of the present day; but we conceive that, like his colleague, *Dr Tweedie*, he ought to have been satisfied with this statement, and should not have called in question the reality of the existence of the adynamic state and the necessity of always regulating the treatment according to its degree in particular epidemics and particular cases of each epidemic.

Lastly, We need do little more than turn to our review of *Dr Burne's* work (xxx. 414,) for the expression of our opinion as to the relation in which continued fever stands to local inflammation. Most of the arguments against the views of *Dr Clutterbuck* on the one hand, and *M. Broussais* on the other, are stated in *Dr Smith's* treatise with great force and with the accuracy of a thinking observer. But his peculiar views, as to the relation which the local phlegmasiæ of fever bear to the primary disease, have shut him out from one argument, which is more decisive of the question than all the rest put together. And this is, that fever not infrequently occurs without any local inflammation from the beginning of it to the end. This argument is succinctly and, we conceive, powerfully stated in our review of Dr Burne's work, which we are sorry to find does not appear to have come under our author's notice. Inflammatory fever certainly occurs often in this city without any local inflammation whatever (xxx. 415, 416.) We are likewise equally certain that we have seen adynamic fever without local inflammation, and have at this moment a distinct instance of the kind under treatment. But it must be admitted that in the adynamic type of fever, instances of the absence of local inflammation are much more rare than in cases of inflammatory fever; and for the obvious reason, that the inflammatory form of fever generally lasts a much shorter time than the adynamic, so that there is not the same opportunity for the disordered state of the circulation to come into play as the exciting cause of local disease.

These statements, we conceive, are called for in consequence of the peculiar opinion which *Dr Smith* advocates as to the connexion between fever and local inflammation. For he maintains that local inflammation in one organ or another is an essential part of fever, that it is indeed a secondary affection, but nevertheless of invariable occurrence, and therefore essential. This opinion is undoubtedly incorrect. In the fevers of other cities and other periods, as has been already stated, the febrile state is often enough seen without any local inflammation; and we suspect that cases of the kind sometimes also happen in London, and have perhaps even occurred under the author's own eye. At least we have the authority of his colleague, *Dr Tweedie*, for thinking so. "I am aware," says he, as if he had anticipated the views of his fellow-labourer, "that many distinguished pathologists not only doubt, but positively deny the existence of what has been termed simple fever, that is, fever without evident signs of local inflammation. On this point, I may state that I have daily opportunities of observing cases, which correspond with the description of simple fever of many writers, in which there is no preponderance of action in any organ that can be detected by symptoms." * * * "Of the whole number of cases which occurred at the Hospital within the period of this report (1828–9) more than 100 came under the description of simple fever; that is, the disturbance in the system was general; there was no evidence by symptoms of an affection either in the head, chest, or belly," (P. 56.) We have already given our own testimony to the same purport in respect to the fever of Edinburgh, and we are supported in the statement by the experience of *Dr Alison*, as described in his paper formerly quoted (xxviii. 251).

We have now considered the leading general principles which Dr Smith endeavours to inculcate respecting the pathology and nature of fever. We apprehend the conclusion must inevitably follow, that in the only parts of the work which are clearly novel, his opinions are not borne out by facts. They may possibly correspond with the observations made in London during the last four or five years. But when a writer undertakes to give an account of fevers in general, his view ought not to have been restricted within so narrow a compass either as to time or place.

In order to convey an adequate idea of the chief merit of this treatise, however, it will be necessary to give some account of the pathological dissections with which it abounds. But this will be better done by taking into consideration at the same time the statements of *Dr Tweedie* on the same subject. Previously to doing so, it is but justice to draw a short outline of that gentleman's researches on the points already discussed.

Dr Tweedie has enjoyed the material advantage over his colleague, not only, we believe, of a longer period of service at the Fever Hospital, but likewise of having observed the fever of this city as it prevailed in our Hospital, of which he was at one time a resident medical officer. We shall not pretend to say, whether it has been owing to the circumstances now mentioned or to a less craving thirst for novelty that he has been indebted for his superior success. But it certainly appears to us, so far as we may judge by a comparison of our own experience here with the narrative given by him of his observations in London, that his opinions are more consonant with nature than those of *Dr Smith.*

He considers his experience entitles him to conclude that fever is primarily a general disease; that local inflammation is a secondary affection, of extreme frequency, yet not always present; that an adynamic state of the constitution is often observed in the fever of London, but that it is neither so frequent nor so great in degree as *Dr Burne* and some others wish to inculcate. It is unnecessary here to go over the grounds on which the author adopts these views, as we have already had occasion to state them in the review of Dr Burne's treatise.

Dr Tweedie has made some interesting and valuable observations on the exciting causes of fever. Among these he assigns, with the most esteemed modern authors, the first place to contagion; and in support of his principles he mentions some very pointed facts partly taken from the paper of *Dr Alison*, published in this Journal in October 1827, and partly derived from his own experience. We have repeatedly had occasion to advert of late to the doctrine of contagion, and the unbelief of certain physicians in the existence of such an exciting cause of disease; and we have endeavoured to explain what, according to the principles of evidence, appears to constitute valid arguments for the determination of this important question. We have already stated that, in our opinion, facts have been adduced in favour of the contagious nature of the fever of this country, over which no effort of scepticism can throw a shadow of doubt or uncertainty. To these the following incident mentioned by *Dr Tweedie* is a valuable addition, and well deserves to be generally known.

" The London Fever Hospital is placed in an open space situated in the vicinity of the metropolis, close to the Small-pox Hospital. Both these establishments stand in the centre of a large field, where the production of *malaria* is extremely improbable. I can state from the most authentic sources, that every physician, with one exception, who has been connected with the Fever Hospital, has been attacked with fever during his attendance, and that three out of eight physicians have died.

"The resident medical officers, matrons, porters, laundresses, and domestic servants, and every female who has ever performed the duties of a nurse, have one and all been invariably the subjects of fever. * * * *

"The fever hospital is asserted by a few, (who deny the doctrine of contagion, but who cannot get over these facts by any thing like plausible reasoning,) to be surrounded by malaria, to which is ascribed the prevalence of the disease among the medical attendants and domestics; but here again they are met by an unanswerable argument. The Small-pox Hospital adjoins the Fever Hospital; in fact, is situated within a few yards of it. If malaria prevailed around the Fever Hospital, it is to be presumed this invisible agent would also produce fever in the medical officers and domestics of the adjoining hospital, both buildings being built on the same lawn. But on inquiry, I am informed by Dr Gregory, physician to the Small-pox Hospital, that no case of genuine fever has occurred among the medical officers or domestics of that institution for the last eight years, the period of his appointment. From repeated inquiries, I can also state cases of fever among the nurses of other large general hospitals in London are scarcely ever observed; and I can assert that the nurses of the Fever Hospital are not exposed to more severe duty than those filling similar situations in other hospitals. The frequent occurrence of fever among the residents of the fever hospital cannot therefore be ascribed to over-exertion in the discharge of their duties."—Pp. 88—90.

To the observations on contagion some useful hints are likewise annexed on the other exciting and predisposing causes,—such as scarcity and privation, atmospheric influences, impure air, and malaria. In reference to the first of these, he mentions an interesting circumstance, that during the year comprehended within his Report, among a great variety of artizans, there was not a single butcher admitted into the fever hospital. The subject of malaria is merely touched on, that he may have an opportunity to express his dissent from the absurd views which have been broached on the subject by a few pretenders to medical knowledge, and too credulously adopted by many extra-professional persons. In illustration of the excessive length to which some of the latter class have carried their faith in the doctrine of local malaria, he tells us he has "known individuals on whom such an assumed cause of the disease has had such an impression, that they have absolutely thrown up their houses, and retired from London!"

On the subject of the mortality of continued fever, *Dr Tweedie* has supplied some important information from the records of the fever hospital of which he has the charge, and also from those of the Manchester House of Recovery. These documents are well worthy of being made generally known, and with that view they are here transcribed.

Year.	London mortality one in	Manchester.	Year.	London mortality one in	Manchester.
1816	11.8		1823	6.11	6.33
1817	12.25		1824	6.0	6.83
1818	5.75	11.75	1825	5.1	6.4
1819	6.72	9.5	1826	5.34	6.66
1820	9.97	8.25	1827	7.25	9.86
1821	5.04	8.0	1828	7.22	10.25
1822	6.18	7.			

The very alarming mortality here exhibited in the London Hospital is partly to be ascribed to the advanced period of the fever at which the patients usually apply for admission, and partly to the disease being actually of a worse kind in London than elsewhere. In Edinburgh, during the height of the last epidemic, from November 1826 to June 1827, out of 1570 patients treated in the Infirmary and Fever Hospital, 153 died, or one in 10.33, which is not much more than half the London mortality for the same period.—It is observed in every fever hospital, that a very considerable proportion of the deaths occur within a few hours after admission, and consequently must not be considered as occurring among the cases which are admitted while under the influence of treatment. *Dr Tweedie* has illustrated this fact very well by his experience in the year 1828–9, during which 19 out of 73 cases proved fatal within 36 hours. —His results as to the mortality at different ages of life are curious, in so far as they differ from most preceding observations in assigning a very high mortality to the age of childhood. On referring to the paper of *Dr Alison*, (xxviii. 249,) it will be seen that when the mortality between the ages of fifteen and thirty is one in $13\frac{1}{2}$ of the sick, that under the age of fifteen is only one in $41\frac{1}{2}$, that between thirty and fifty one in $5\frac{1}{2}$, and that above fifty one in $2\frac{1}{2}$. On classifying *Dr Tweedie's* cases in the same manner, we find that of 521 cases admitted in the year 1828–9, 35 were above fifty, 132 under that and above thirty, 285 under that and above fifteen, and 66 under fifteen; and that the mortality under fifteen was so high as one in $7\frac{1}{5}$, between fifteen and thirty one in $9\frac{1}{2}$, between thirty and fifty one in $7\frac{1}{5}$, and above fifty one in $2\frac{1}{2}$. *Dr Smith's* experience is to the same purport, but does not give quite so unfavourable an average to the period of childhood. Of 2537 cases admitted in 1825–6–7–8, 429 were under fifteen, 1562 between fifteen and thirty, 449 between thirty and fifty, and 97 above fifty; and of these several denominations the mortality was one in $11\frac{1}{5}$, 7, 5½, 3¼. There must surely be some very special cause for this high mortality in London under the age of fifteen.

We shall now attempt to give a succint view of what the reader will probably agree with us in thinking the most valuable part of the two works at present under consideration,—the facts which *Dr Tweedie* and *Dr Smith* have collected on the pathological appearances after fever. In this department we believe their treatises are richer and more complete than any work hitherto published in the English language.

Dr Tweedie gives the particulars of 54 dissections, and *Dr Smith* has related the heads of 110, of which about one-fourth are also in *Dr Tweedie's* list.

Of *Dr Tweedie's* cases we may set aside thirteen, as examples of idiopathic local inflammations, namely, two of pleuro-peripneumony, one of peripneumony, one of abscess in the lungs, one of pleuro-peripneumony with pericarditis, two of erysipelas and pericarditis, occurring during convalescence from fever, one of arachnitis, one of hypertrophy of the brain, two of peritonitis, one of intus-susceptio and peritonitis, and one of scarlatina.

Of the remaining cases, we find there are eleven where unequivocal appearances of disease were found in the brain. In five there was great effusion of serosity into the ventricles of the brain, in one to the extent of two ounces, in three three ounces, in one four ounces; and in one of these there was a little pus in the base of the ventricles, and in another an obvious effusion of fibrin or coagulable lymph among the external membranes. In five other cases there was a great effusion of serosity among the external membranes,—so great that no doubt could exist as to the appearance being really the effect of disease, and not merely a pseudo-morbid phenomenon; and in one of the five the fluid formed a sac of twelve ounces in capacity, which compressed and indented the brain. In the eleventh case of cerebral affection, there was excessive redness of the whole substance of the brain, so that it appeared as if stained with port wine. Besides these unequivocal appearances, the brain or its membranes presented an unusual degree of vascularity and turgescence of vessels in nineteen other instances; but of course doubts may be entertained whether such redness is always or even generally a true morbid appearance. In nine cases, the brain and its membranes were either perfectly healthy, or so near their customary state as to turgescence and watery effusion, that it was impossible to say with any confidence that signs of vascular action were present.

In seventeen cases there were evident inflammatory appearances in the chest. Thus five cases presented obvious signs of pleurisy in its acute form, fibrinous effusion having taken place in all, and also serous or sero-purulent effusion in two of them. In all the five cases the pleurisy was united with peripneumony. This was the most common of the thoracic appearances; for besides the five already mentioned, there were seven others in which the lungs were in a more or less advanced stage of hepatization. The hepatization was sometimes very limited, but at other times extremely extensive. In one case apoplexy of the lungs was found. In eight there were tubercles, which in some instances occurred along with recent hepatization, and were then generally of the miliary kind.

The appearances of disease in the abdominal organs were extremely various. By far the most common appearance was ulceration of the small or great intestines. This was observed in fourteen cases; and it seems to have answered in every instance the description given of the thickening and ulceration of the Peyerian and Brunnerian glands lately described by *Serres*, *Louis*, *Bright*, *Burne*, and others. In two of the cases the ulceration proceeded to the extent of perforating the gut; but, on account of the oppressed state of the functions of the brain, the symptoms of perforation were not so sudden and violent as they are usually observed to be when the sensorium is less disordered. In four cases there was an effusion of lymph and sero-purulent fluid into the sac of the peritonæum, two of which were the cases of perforation just mentioned. In the two others the mucous membrane of the intestinal canal was healthy; and in one there was also great effusion of lymph and turbid serum into the chest, with extensive hepatization of the lungs. In six cases the intestines were internally so red as to deserve the name of inflammation. In four cases the mesentery was tuberculated; and in one this disease occurred without ulceration of the mucous membrane of the intestines. The liver was indurated in four individuals. The spleen was flabby in four, indurated or tuberculated in two, and in another it contained a large abscess. In four cases the liver was indurated.

Dr Smith's account of the morbid appearances corresponds in many respects with that now given, but differs in so far as he represents the signs of inflammation to be even much more frequently present than consists with his colleague's observation.

As to the head, he says that the arachnoid membrane is seldom or never free from disease, being always either more vascular than natural, or opaque, or raised by a gelatinous effusion, or united to the pia mater or dura mater,—that the dura mater is also often vascular, and likewise the pia mater,—that the brain itself is seldom or never in a healthy state, its substance being vascular, or harder than usual, or unnaturally soft, sometimes even to *rammollissement*, the pituitary gland very constantly softened, and the ventricles of the brain distended more or less with serosity, in which flakes of lymph or portions of pus are sometimes mixed. The membranes of the spinal cord, he also states, are very generally vascular and infiltered with fluid. In the chest, he mentions inflammation of the mucous lining of the air-passages as "the most constant and most characteristic" of all the appearances in this cavity which present themselves after death from fever; and this disease, which consists in dark redness and thickening of the membrane, with effusion of mucus, occasionally puriform, he considers to be "specific and uniform,"—though for what sufficient reason it is not very easy to discover. Besides this disease, he has also occasionally found, like *Dr Tweedie*, redness, fibrinous effusion, and sero-purulent secretion in the pleural sac, gorging, hepatization, and tubercles of the lungs. In the abdomen, he has invariably found the whole organs darker and more vascular than natural,—the mucous membrane of the intestines unusually vascular in a great proportion of instances, very often thickened by a deposition of adventitious matter among the mucous glands, and also very frequently ulcerated;—the mesenteric glands enlarged or suppurated according to the extent of disease in the intestines,—the spleen changed in appearance and structure in "almost every case of genuine fever," the colour being unusually dark, and "its substance on being slightly touched with the finger, breaking down into an almost fluid mass,—the pancreas also almost always diseased, namely, firm, indurated, and sometimes even partially cartilaginous,—the stomach occasionally red in its villous coat, which is then sometimes thickened and softened, but much less frequently than appears to have been noticed in France,—the liver not often diseased, and the disease confined chiefly to unusual darkness or softness,—lastly, the peritonæum in a few cases presenting all the signs of acute inflammation."

Such is a summary of the observations of *Drs Tweedie* and *Smith* regarding the pathological anatomy of fever. In commenting on their description, we shall, in the first instance, allow the former gentleman to correct his colleague, who appears to have been somewhat too active in the discovery of diseased appearances. *Dr Tweedie* states distinctly, that in numerous instances various organs of the body, which *Dr Smith* thinks he has invariably found to exhibit after death proofs of reaction during the course of the fever, were really to all appearance healthy; and this we are quite prepared to believe, as it is consistent with our own frequent observation. *Dr Smith* seems to have been satisfied he had discovered traces of inflammation whenever he could observe a little unusual vascularity, a little darkness of a membrane or parenchymatous tissue, or a little watery effusion. Now he ought to have been aware, that all of these appearances may take place to a great extent without any disease, and that they are apt to occur as pseudo-morbid appearances chiefly in those diseases which, like most instances of fever, are terminated by a long stage of agony, and, consequently, a slowly increasing asphyxiated state of the blood. We recommend him to study the researches of *MM. Rigot* and *Trousseau*, and those of *M. Billard*, formerly noticed in this Journal; and he will then perhaps be satisfied that he has greatly overrated the frequency of inflammatory appearances in the brain, the bronchial membrane, and the inner coat of the intestines.

But after making every allowance for the oversight here pointed out, there will still remain in his work, and in the more measured description of Dr Tweedie, ample evidence of the extreme frequency of the appearances of advanced inflammation in various organs of the body after fever, and more especially of inflammation in the brain, the pleura, the lungs, the spleen, and above all the intestines. And the statement made by both, that a body is rarely examined without the discovery of inflammatory appearances in one organ or another sufficient in extent to account for death, is probably not very remote from the truth, so far as the late fever of London is concerned.

Here, however, it is particularly necessary to caution the reader, that the general fact thus established with respect to the fever of one place must by no means be hastily applied, as Dr Smith appears willing to do, to fever generally, or to the fever of any other particular place. It certainly appears that a diseased state of the intestines, though common in London, is far from being so common there as the late researches of the French pathologists have proved it to be in their country. But it is a circumstance of still greater interest, that the observations of *Dr Tweedie* and *Dr Smith* show inflammation and ulceration of the intestines, and the consequences of inflammation of the internal organs generally, to be much more frequent in the present fever of London than in that which for some years past has been witnessed in Edinburgh. In proof of this, it will be sufficient to turn to the statements of *Dr Alison* in the 28th volume of this Journal, where it will be seen that marks of cerebral excitement are not uncommon, but pleuritic effusion very rare, pulmonary turgescence and hepatization also far from being frequent, and inflammation of the peritonæum, or ulceration of the intestines, very seldom witnessed. Our own experience of the Edinburgh fever, since the publication of *Dr Alison's* paper, is exactly to the same effect, except that ulceration of the intestines has been observed in one or two cases. *Dr Alison* also notices, what every hospital physician here must have had occasion to observe, but which the physicians of the London Fever Hospital seem seldom or never to witness at present, that not a few patients die in fever from simple *asthenia* or exhaustion of the powers of life, without any particular organ being affected. These striking differences in a disease essentially of the same nature, and occurring in localities where naturally the difference might have been expected to be the converse of the actual fact, are well deserving of notice, but by no means easy to explain.

In conclusion, we have to present a short view of the treatment now pursued by the physicians of the London Fever Hospital.

The principles of practice laid down by *Dr Tweedie* constitute perhaps the best abstract that has been for some time published of the modern treatment of continued fever. As the author considers he has proved, that the type of the London fever is much less frequently adynamic or *asthenic* than Dr Burne and some other physicians of the metropolis have supposed, it is natural to expect a very different practice recommended in the present work. Accordingly, the great object of the Fever Hospital physicians has been to subdue local inflammation.

For this end the main resource was blood-letting. *Dr Smith* endeavours to enforce the advantages of blood-letting in terms which we cannot help denominating extravagant. His conviction of the unjustly low estimation in which it is held by his fellow-practitioners in London may have led him to resort to stronger language than he might otherwise have employed. But certainly his commendation appears to us too exclusive and violent. Even *Dr Tweedie's* account of its benefits, though more measured and cautious, is yet sufficiently commendatory too. "The experience," says he, "of the decided benefits which result from blood-letting in the treatment of the patients at the fever hospital, fully warrant me in affirming that there are few cases which are not materially benefitted by its judicious employment, and I had too often reason to regret that it had not been performed at the commencement of the disease."—P. 166. He admits, however, which Dr Smith seems unwilling to do, but which no sober-minded physician who has dispassionately observed the progress of a few epidemics would hesitate to state explicitly, that this "is not a remedy, which should be indiscriminately adopted, as if fever were identical with inflammation;" and "that an epidemic may appear which will not bear the same bold treatment which has been recommended" in his Report. This is all as it should be. In illustration of the correctness of these cautions we may observe, that in the epidemic of this city in 1817–20, free blood-letting was a favourite remedy, and undoubtedly most beneficial, but that in later years the type of the fever, as we have already stated, has been so manifestly changed that blood-letting has in general been much less easily borne, and far less frequently called for. We have no doubt whatever, that the reason why this remedy has appeared so much more essential an agent in the treatment of the late London fever, is simply the greater violence and frequency of local inflammation. We are fully prepared, therefore, to allow, that the practice of *Dr Tweedie* in the London fever has been judicious. The following statement will enable the reader to judge whether or not it has also been useful. Of 521 patients, 210 lost blood by venesection; and the average quantity of blood drawn from each was twenty ounces. Of these 64 lost blood also by cupping or leeches to the average amount of ten ounces each. Of the whole 210, twenty-three died, that is *one in* 9.13. Now the general mortality for the period comprehended in the Report is *one in* 7.13. Here then is a manifest advantage in favour of the treatment by blood-letting; which, however, is still better shown in relation to the mortality of those who did not undergo venesection. These amounted to 240, and the deaths were thirty-six, or *one in* 6.66. This evidence is so far satisfactory; but to render it completely so, it would have been desirable that the author had mentioned how many of the 240 were not bled, simply because the disease was advanced beyond the reach of blood-letting or any other active remedy. The necessity of this condition forming an element in the calculation is shown by the mortality among those who lost blood only locally; for of 70 of that class no fewer than fourteen died, being a mortality of *one in five*. In the same manner, as it would be wrong to infer from this result, that local blood-letting is a pernicious remedy, instead of inferring that it was employed in the worst of cases, where evacuations were required to subdue inflammatory action, but could not be vigorously practised on account of the exhausted state of the system,—so it appears too hasty to conclude that the favourable mortality among those who were freely bled depended on the treatment, and not in some measure on the stage of the disease and strength of the patient's constitution being favourable to their contending prosperously with the disease. Still, however, it must be kept in view, that the great source of mortality in the London fever has been local inflammation, and it is therefore reasonable to suppose that the practice of blood-letting would have a favourable influence on the mortality by subduing local inflammatory action.

In respect to this remedy we have only further to add, that Dr Tweedie does not allow great latitude as to the stage of the disease in which it may with propriety be practised. Of the 210 cases, 96 were bled on the fifth day of fever, or before it, 74 before the ninth, 30 before the fifteenth, and only 10 after the fourteenth.

Of local blood-letting both authors speak deservedly in very favourable terms. *Dr Tweedie* considers it the best mode of abstracting blood in the bronchitis of fever; and prefers cupping to leeches.

Emetics he discards as useless. Diaphoretics he also rejects as inert. The latter remedies, he observes, have enjoyed "a more lasting reputation than their real merit deserves. The practice, in itself perfectly inert, has had the injurious effect of excluding more powerful measures."—"I must say I seldom prescribe diaphoretics in fever from a conviction of their inefficacy."—"I include in these remarks the whole tribe of saline remedies." In these views we coincide with him entirely. From a wide experience of their inutility during the period when the fever hospital of this place was lately open, we are convinced with *Dr Tweedie* that they serve no other end than to show the *nimia medici cura.* These remarks, however, must not be understood as excluding the use of antimony in fever complicated with bronchitis, in which we also agree entirely with the author that no remedy is more salutary and rapid in its operation than tartar-emetic in large doses.

The cold affusion is also considered by *Dr Tweedie* an insignificant remedy. Notwithstanding the high encomiums of Dr Currie, "I never," says he, "witnessed the fever extinguished by it, nor, so far as I was able to judge, was its course ever shortened." The chief reason of the inconsistency between the statements of *Dr Currie* and those of late physicians on this subject is, we apprehend, that the late epidemics have not been attended with the violent general excitement and increased heat which were remarked in the fevers in which the cold affusion was most successful. Cold and tepid sponging have been used by the physicians of the London Fever Hospital with more apparent benefit. In a few cases of violent determination towards the head the cold dash has been resorted to with great success. *Dr Tweedie* commends it, and *Dr Smith*, as usual with him when he gets hold of a good remedy, talks of it in rapturous terms. We cannot conceive where the latter gentleman discovered that this treatment is "little known and less appreciated." It is most simply applied by raising the patient a little in bed, with his back towards the edge and his head half inclined over an empty vessel, and then pouring cold water from a large jug on the scalp, the stream being gradually raised as the patient can bear it. " This mode," says Dr Tweedie, " produces a great shock at first, but the relief it gives invariably induces the patient to request its frequent application. I employ it constantly both in hospital and private practice, and certainly with the most excellent effects."—P. 193.

Wine has been sometimes used at the Fever Hospital, notwithstanding what we have said of the prevailing type of the London fever. In fourteen cases of typhus out of 521 of fever it was given, evidently with the best effects.

Purgatives have been given freely, but only to procure the discharge of accumulated fæces, and have never appeared to induce, as the French physicians fear, the inflammatory affection of the intestines which so often attends the fever of London and Paris.

Mercury has been occasionally used as a mercurial, and with apparent advantage. " I am frequently," says Dr Tweedie, " in the habit of ordering the external application of the mercurial ointment in fevers where the general powers are feeble, and the local inflammatory symptoms have assumed a low character either from the neglect of proper measures, or from the protracted nature of the disease. It would appear that the febrile action forms a protecting power against the influence of mercury, as its action on the system does not take place till the fever subsides ; hence I always regard the early effect of mercury as a favourable circumstance in fever."—P. 185.

We have reserved till the last what we have to say of *Dr Bow's* " Notions of the Nature of Fever," because they have no connexion whatever with the observations of *Drs Tweedie* and *Smith*, being entirely of a theoretical nature.

Dr Bow conceives that the essence of fever consists in the reciprocal exhaustion and restoration of the nervous energy in various parts and textures of the body,—that the first phenomenon in the train of events which constitute general fever is diminution of the general nervous power,—that in consequence the capillary vessels lose their tonicity, become distended, and permit the entrance of more blood than in the healthy state, so that congestion is induced,—that the increased afflux of blood, by supplying too much animal heat to the capillary textures, acts as a cause of excitement by which the force of the circulation is augmented,—and that the want of a proper supply of nervous energy, by which it is assumed that the secretions are in a great measure regulated, is the reason why the various secreted fluids put on an unhealthy appearance. He further maintains, that when any part is more oppressed than another by a proportionally greater deficiency of nervous power, the true method of treatment is to diminish the nervous supply in adjacent parts by artificial means. In this manner he accounts for the frequent advantage he has observed to flow from the external use of opium in the delirium of the advanced stage of fever, and in the cough and dyspnœa both of that disease and of croup, asthma, and other pectoral disorders. The opium diminishes the nervous excitement and supply of the skin, so that a larger supply is distributed to the internal parts which are suffering from its deficiency.

It is unnecessary for us to follow the ingenious author through all the ramifications of his theory, as it would be extremely difficult for the reader to comprehend his exposition of it without a much fuller analysis than it is in our power to give. Neither would it serve any useful purpose if we were to express our opinion on a point so purely theoretical farther than by stating, that his arguments are well put together, his narratives always clear, and his views unquestionably ingenious. His remarks on the advantages to be derived from the external employment of opium by way of friction in fever and various febrile diseases are well worthy of notice, and of being subjected to the test of experiment.

ART. II.—*The Anatomy, Physiology, and Diseases of the Teeth.* By THOMAS BELL, F. R. S., F. L. S., F. G. S., Member of the Royal College of Surgeons in London, &c. &c. London, 1829. 8vo. Pp. 329.

HOWEVER general be the practice of authors in finding an excuse for publication in the deficiences of the labours of their predecessors, it is by no means without reason that Mr Bell asserts, that a practical work on the anatomy and diseases of the teeth, founded on the improved state of knowledge, physiological and therapeutic, is still a desideratum in the medical literature of England. Whatever be the merits of the writings of Hunter, Blake, and Fox, and they are by no means inconsiderable, they were originally little more than sketches, and at present they have fallen decidedly behind the state of knowledge in this department of the art of healing. In accuracy, indeed, their anatomical descriptions remain unrivalled, and will always form a valuable source of information to the dentist. Much of their physiological and pathological matter also is worthy of preservation, in being the first attempts to fix the treatment on a rational foundation, and to apply to this, as to other branches of surgery, general scientific principles. Notwithstanding these recommendations, however, they have in the natural progress of things become defective as works to guide the surgeon and surgeon-dentist. It would indeed be a poor compliment to those who, since the time of these authors, have cultivated this department of surgery, if it could be asserted that they had been unable to improve; and it would be almost absurd to suppose, that in the course of more than thirty years there was nothing to amend. The fallacy of such ideas is demonstrated by daily experience; and perhaps few of the operative and manual branches of the art have undergone so much amelioration, and have been so completely converted from a mechanical and empirical into a scientific and liberal art.

This result must indeed be in a great degree attributed to the example set by the authors above-mentioned, who certainly have the merit of first reducing to fixed principles the rules of an art previously too mechanical; and it is perhaps rather extraordinary that so long a period has elapsed before any English surgeon has undertaken to complete what was so well commenced by Hunter, Blake, and Fox. The attempts of Delabarre and a few other foreign surgeons are certainly so far creditable; that they also have applied anatomical and physiological principles to illustrate the diseases of the teeth and their treatment. But to these works the smallest objection is, that they belong to the literature of a foreign country, and therefore can never be expected to become manuals for the English dentist. A more serious objection is, that whatever pre-eminence in the mechanical department of the dentist the French and German surgeons may have acquired, their works are much inferior to the English in the general merit of their therapeutic measures. Excessive confidence in mechanical contrivances, with proportional ignorance of the resources of nature—extreme dexterity in manual operations, with little attention to the means by which they may be superseded—great attention to local causes of disease, to the neglect of those which are constitutional,—are the leading characters of their theory and practice. So long as the dentist looks on the teeth as insulated organs, unconnected with the system at large, uninfluenced by constitutional causes, and exerting little or no re-action on the constitution, so long will he fail to recognize the true nature of their morbid states, and to distinguish the most rational means of prevention and cure. It is the connection of the physiological properties and pathological states of the teeth with the remedial means that alone can raise the profession of the dentist above the rank of a mechanical art. We do not wish to be understood to assert that this empirical system is the character of all the foreign works without exception, and that none but English surgeons are scientific dentists; but, generally speaking, it may without injustice be said, that in no foreign work is it possible to recognize throughout, that constant attention to the influence of constitutional agents on the state of the teeth of which English authors have very rarely lost sight.

It is evidently under the influence of such views as these that Mr Bell has composed the present work. He is manifestly not a mere dentist or mechanical tooth-surgeon; and the great merit of his work is, that, in imitation of the valuable models by which he was preceded, he combines the accomplishments of the well-educated scientific surgeon with those of the ingenious dentist. He has been at great pains to develope accurately the structure and properties of the teeth, and to apply the knowledge thus acquired to the explanation of their diseases, and the most probable modes of preventing and curing them. Nor has he undertaken this task without that cautious preparation which is at present so little in fashion, and the neglect of which is too often mistaken for indication of original talent and novelty of views. Not only has he studied the subject with assiduous personal attention, but he has availed himself with much judgment of the labours of his predecessors; and while he canvasses their merits with great liberality, he candidly admits their services, and rectifies their errors. It is indeed a pleasing circumstance in the composition of the present volume, that while the author speaks with enthusiastic veneration of the labours of Hunter, Blake, and Fox, and freely and willingly acknowledges their merits, he can rectify their mistakes, and supply their deficiencies, without severity of criticism, or triumphant exposure of their failings.

The work consists of two parts;—the first, on the Anatomy and Physiology; the second, on the Diseases of the Teeth, and the means by which their appearance is to be prevented, or their progress resisted and checked.

On the former subject we shall make no apology for not recapitulating what is found in most anatomical works. It may be well, however, to select for notice some few points placed by Mr Bell in a clearer light than heretofore, as illustrated by some new or additional facts.

The first important question discussed by Mr Bell is that of the organization of the teeth. This, it is known, has been, upon the grounds of mere anatomical evidence, denied, or at least doubted, by some authors ambitious of the distinction of novelty and originality. These innovators we distinguish in this manner, because we remember no professed anatomist of any observation, notwithstanding the authority of Mr Lawrence and Mr Blainville, who has ever doubted the organization of the teeth; and we are inclined to think that Mr Bell overrates the extent of the prevalence of this heresy. Certain it is that the enamel alone can be maintained, with any shadow of truth, to be inorganic; for the organic nature of the osseous portion of the teeth may be demonstrated in a variety of modes. Admitting the inorganic character of the enamel, which appears to be a peculiar secretion, all the best anatomists agree in representing the bone of the tooth to be liberally supplied with vessels. Portal even adduces the fact of the vascularity of this substance, to show that its inflammation ought not to be matter of surprise. Bichat questions the organization of the enamel only; and Soemmering in like manner describes the bony part as organized like other bones. That the interior part of the teeth is organized, Mr Bell thinks is to be inferred, first, from the vascular connection subsisting between it and the periosteum, and the internal membrane or pulp of the tooth; secondly, from the circumstance of red patches being found in the bony part of teeth broken after extraction, where there had been previous inflammation; and, thirdly, from the teeth being tinged yellow in jaundice, and of a deep-red in persons destroyed by drowning or suspension. The two latter phenomena he justly adduces instead of artificial injection, which no anatomist has hitherto succeeded in accomplishing. The injectibility or non-injectibility, indeed, of the teeth is a point of little moment to the question at issue. We believe no one has doubted that cartilage, tendon, and ligament are organized; yet no anatomist has succeeded in injecting these substances in the adult. With these textures, and with the teeth, it appears to be a peculiar arrangement, that during the process of growth they are vascular, and, if not perfectly injectible, at least receive minute vessels. After formation, however, these vessels shrink and become so minute, that they neither can be seen by the glass, nor traced by means of the syringe. It is hence that these textures, which are totally uninjectible in the adult, seem reddish and vascular in young animals.

In demonstrating the existence of nerves and absorbents as elements of organization, Mr Bell, we think, is less successful. The latter class of vessels, that is lymphatics, cannot be regarded as essential to organization, for many organized parts exist, so far as anatomical evidence goes, without them. Nor is their existence rendered even probable by the existence of the process of absorption; for this we know may be performed by veins as well as lymphatics; and in all probability the former class of vessels are the general agents of absorption throughout the system.

The existence of nerves, though probable, is, however, equally problematical; and the argument by which their presence is inferred, if rigorously examined, will amount to little more than the paralogism denominated reasoning in a circle. First, it is said as a major, the occurrence of pain implies sensation, which again implies the existence of nerves. The minor proposition is, the teeth are often the seat of pain, and consequently the teeth must have nerves. Now, without denying the well-established fact, that the teeth are often the seat of acute pain, and that this pain implies sensation, we think it is more than doubtful that pain indicates the presence of nerves. It is quite certain that there are in the animal body textures in which nerves have never been traced by the most careful anatomists, and which, nevertheless, become the seat of most acute pain when inflamed. This is the case not only with the serous membranes in general, but with several of the fibrous tissues, for instance periosteum, tendon, and ligament, all of which we know, though quite insensible in the healthy state, become exquisitely painful when the seat of inflammation. Now, it is not only absurd to suppose that nerves are developed by the process of inflammation, but gratuitous to imagine that the new sensation indicates the existence of nervous filaments. The whole doctrine is, indeed, conjectural and hypothetical; and it is quite as rational, and a great deal more natural to say that the textures, as organic substances, are endowed with certain properties of sensation, by virtue of which they become painful when inflamed, as to assert that they derive this property from the nerves. From the fact, indeed, that nerves are themselves insensible, and that the parts most freely supplied by them are not the most exquisitely sensible, it may be inferred that nerves do not communicate sensibility. Their chief use seems to be rather to regulate this property, and preside over its distribution.

The application of these principles to the case of the teeth is obvious. The pain resulting from filing the bone, while none is felt from the destruction of the enamel, cannot be regarded as a conclusive proof that the former is provided with nerves, and indicates merely that the inherent sensibility of the tooth is morbidly augmented. But while it must be regarded as still doubtful that the bony matter even of the teeth receives nerves, it cannot be denied that the pulp or internal membrane receives some filaments of this kind. In no instance, however, have these been traced into the substance of the tooth. Upon the whole, though we do not conceive that the presence of nerves or absorbents in the teeth is rigidly demonstrated, yet the existence of these textures we hold by no means requisite to establish the organic character of the teeth, and we willingly concede to Mr Bell, that while he fails to prove the former, he succeeds completely in establishing the latter doctrine. He nevertheless allows that the vitality thus inferred is of a much inferior degree to that of other organic tissues, and especially of bones; and he willingly admits this peculiarity as exercising considerable influence on the diseases incident to these organs.

The subsequent observations on the nerves of the teeth are not justly denominated; and hence the author does not, as might be expected, specify a single nervous twig going to the teeth, but enumerates those only which contribute to the sensibility of the face, lips, and mouth. These, it may be observed, are not, properly speaking, nerves of the teeth; and the chief use of the knowledge of these branches is to explain the sympathetic or rather contiguous effects produced by disorders of the teeth.

The formation of the teeth or the manner in which they are developed is the next point deserving attention. Dr Blake was the first author who gave a correct view of the several steps of this process; and it was afterwards explained in an interesting manner by Bichat. By the former, the dentiferous sac is erroneously represented as a propagation of the periosteum of the jaw. The latter more justly considers it an insulated follicular membrane formed like a shut sac, but attached externally to the parietes of the *alveolus* by means of minute vessels, and secreting from its inner surface the rudiment of the future tooth. One part of this shut sac, particularly vascular, constitutes the pulp, and from this the tooth is formed, first at the crown, by deposition of osseous matter, which gradually extends to the root, and eventually by the deposition of enamel at the surface, and by the retraction and closure of the interior canal.

The description of Mr Bell is as follows:—" This membrane, the use of which will be explained hereafter, is not attached to the pulp, except at its base, where the vessels enter; nor originally to the bony rudiments of the teeth, but only loosely surrounds them; its external part is, however, most intimately and inseparably connected with the gum, from which source it is supplied with vessels and nerves, while the pulp and its proper membrane derive their organization immediately from the dental branches. The bone is first deposited on the points of the teeth, and assumes by degrees the appearance of a thin shell, covering the pulp and its proper membrane, which retire as it were as the bone continues to be deposited. It is obvious, therefore, that the bony shell is interposed between the pulp, with its membrane and the investing sac. As the pulp is originally of nearly the same form as that which the crown of the tooth is destined to assume, ossification commences on as many points as the future tooth will have eminences on its surface: Thus the *incisores* and *cuspidati* commence with one point, and the *molares* with several. When the shell of bone has proceeded so far as nearly to cover the original form of the pulp, the alveolar processes having become more developed; and consequently the *alveoli* deepened, and more perfectly separated by the growth of the transverse plates, the pulp elongates at its base to form the root; in those teeth which have only one, a conical shell of bone is formed round the pulp, which continues to increase in length in proportion as the ossification advances; while in those which have several roots, a division of the pulp takes place at this part into a corresponding number of processes. When, therefore, the external lamina has advanced to the part where this division occurs, a process of bone is sent across from one side of the shell to the other, between the divided processes of pulp, and ossification is then continued in each respectively."

"It will be seen from the foregoing account of this curious process, that the outer lamina of bone is first completed, and that lamina after lamina is deposited one within another, the pulp still receding, until at length there remains only the permanent cavity of the tooth lined with its proper membrane, and filled with the remaining portion of the pulp, which now serve as the bed in which the vessels and nerves ramify previous to entering the bony substance of the tooth."

These different circumstances have been already well described by Bichat * who, though he probably copied both from Hunter and from Blake, has the merit, nevertheless, of exhibiting a very neat general view of the whole process. The account now given by Mr Bell is, however, useful in being the most complete and perspicuous before the English reader. His description of the formation of the enamel, though formed on the model of those of Blake and Fox, is nevertheless more minute than the accounts given by these authors.

"When the bony shell has extended as far as the neck of the tooth, the external membrane or sac attaches itself closely to this part, though still loosely investing its body. At this time a remarkable alteration takes place in its substance, which becomes thickened, and much more vascular, particularly the inner layer over the whole internal surface, of which the vessels may be seen obviously enlarged, and capable therefore of receiving a much more perfect injection. The object of this change in the condition of the membrane is to produce the increased supply of blood required for the secretion of the enamel. It is more evident in the permanent than in the temporary teeth, partly from the difference in their size, but doubtless more especially because the demand for the increased determination of blood is greater in the permanent, as the enamel is required to be of much greater proportional thickness."

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* Anatomie Generale, Tom. iii. p. 91, 92.

"It now begins to pour from its internal surface a thickish fluid, which is speedily consolidated into a dark chalky substance, and afterwards becomes white and hardened by more perfect crystallization. This is the enamel. It is first deposited on the points at which ossification had commenced, and by degrees covers, in one continuous layer, the whole crown of the tooth. It appears that in the human subject, and in other animals whose teeth possess but two distinct substances, bone and enamel, the external lamina alone performs the office of secretion; but in those which require a third substance, the *crusta petrosa* of the graminivorous quadrupeds, this is secreted by the external lamina, after the internal had performed its office of producing the enamel."

With the view of communicating a clearer idea than has hitherto been given of the different membranes concerned in this curious process, Mr Bell, in a short recapitulation, distinguishes them into two divisions, the *deciduous* or temporary, and the *persistent*. The *deciduous* are the two lamellæ which, forming the sac containing the rudiments, secrete the enamel from their internal surface, and are then absorbed. The *persistent* membranes are, 1st, the periosteum of the dental cavity; 2d, the periosteum of the root; and 3d, that of the alveolus, of which he regards the periosteum of the root as a reflection. The first, it may be observed, is not, properly speaking, a periosteum; it is merely the membranous remain of the pulp or vascular production, originally employed in the formation of the tooth.

The only other topic deserving notice under this head is the formation of the rudiments of the permanent teeth. These are originally secreted from the pulp during the formation of the temporary teeth, and are contained for some time in the same cavity, which is fitted for its reception by the absorbents, on the same principle on which the roots of the milk teeth are destroyed. This process of absorption, Mr Bell justly refuses to ascribe to mere pressure, because he has witnessed its commencement before the formation of the new sac, and consequently before pressure could take place on the *parietes* of the cavity.

"The absorption does not commence in the smooth surface of the alveolus; but in the cancelli of the bone immediately behind it, where no pressure could operate; and it is only by carefully removing the parietes of the socket, where the excavation is intended to be formed, that the commencement of the process can be observed. By degrees a small recess is thus formed in the parietes of the alveolus, in which the new rudiment is lodged, and this excavation continues to increase with the increasing size of the rudiment, while at the same time the maxillary bone becomes enlarged, and the temporary tooth advancing in formation rises in the socket. The new cell is thus gradually separated from the former one, both by being itself more and more deeply excavated in the substance of the bone, and also by the deposition of a bony partition between them; and the rudiment of the permanent tooth is at length shut up in its proper socket. There is not, however, even now, a total disunion between the two teeth; for, as the temporary one grows and rises in the jaw, the connecting cord or peduncle elongates; and, though the sac from which it is derived by degrees becomes absorbed, it still remains attached to the rest of the temporary teeth, even long after the latter has pierced the gum; and this connection between the temporary tooth, the permanent rudiment, and the gum is thus kept up by means of the cord, through a small opening in the top of the new alveolus, which is seen perforating the alveolar process immediately behind each temporary tooth."

To this mode of representing the phenomena, which is on the whole just, little objection perhaps can be urged. The idea of actual excavation by means of absorption is nevertheless something gratuitous. The cavities in which not only the permanent but the temporary teeth are contained may with equal justice and more probability be ascribed to the process of deposition or growth going on with greater activity at certain points than at others. The jaws as well as the alveolar partitions are in the process of growth and formation. The dentiferous sacs are formed in them at an early period; and where these sacs rest, the process of ossification is either arrested or advances slowly. All round, however, the periosteal vessels are actively engaged in depositing bone; and the mechanical shape which this osseous deposit is to assume is regulated almost entirely by the shape of the dentiferous sacs, round and between which only this deposition proceeds. It hence results that the alveolar compartments are formed, not by excavation, but by deposition round the vascular membranous sacs. Of the cavity for the permanent rudiment, the same may be said. It is not formed by excavation, but by the deposition of bone round the rudiment; and, as the jaw enlarges, the walls of this cavity also enlarge, and give it the appearance as if formed by excavation. The process is throughout one of deposition and moulding, and is indeed one of the numerous examples in the animal body, in which the hardest and most solid parts are shaped and moulded by the softest.

The date of the ossification and eruption of both classes of teeth Mr Bell admits to be liable to very great variation. Some curious instances are recorded of children being born with one or two teeth, while in other cases they have not appeared till the fifteenth or sixteenth month, or even after two years. Of the former variety, besides the well-known instances of Richard III. and Louis XIV., Haller cites no fewer than nineteen examples of infants born under the same circumstances. * These variations are simply expressions of the different rates at which they burst through the gum. As a general rule, Mr Bell exhibits the following statements:—

From 5 to 8 months,	the 4 central *incisores.*
From 7 to 10,	the 4 lateral *incisores.*
From 12 to 16,	the 4 anterior *molares.*
From 14 to 20,	the 4 *cuspidati.*
From 18 to 36,	the 4 posterior *molares.*

The *irritation and diseases attending the first dentition* are ascribed by Mr Bell to the disproportion between the rate of progress of the tooth, and the necessary absorption of the enclosing parts, causing painful pressure of the gum above and of the pulp below by the tooth. The constitutional disorder which is well known to attend this process assumes different forms. In the mildest the action of the liver is impaired; but in general the disorder proves its own remedy, by the occurrence of moderate spontaneous diarrhœa. In more severe cases, in which the constitutional disorder is extreme, immoderate diarrhœa, with fetid, discoloured, clayey or mucous discharges is a prominent character. The urine is also scanty, turbid, and discharged with much difficulty and pain. The respiration is quick, laborious, and accompanied with cough, not unfrequently simulating pneumonia. Even the nervous system may be disordered, with dilated pupils, incessant motion of the head, and spasm of the voluntary muscles, or even convulsions, and in some instances proceeding to hydrocephalus, or at least idiocy. For the prevention of this train of evils, Mr Bell, though allowing the importance of constitutional remedies, and means operating on the skin and the digestive organs, maintains that the only efficient antidote is the removal of the primary cause of mischief,—the pressure of the tooth,—by dividing the gum. The remedy, we say, from personal experience, is direct and powerful, and merits all the confidence which the author reposes in it.

In speaking of the progress of the permanent and the shedding of the temporary teeth, Mr Bell admits the influence of the process of absorption, but denies the agency of pressure in effecting this absorption. He does not, however, attempt to supply its place by any other, unless it be the hypothesis of anticipation, of which he makes considerable use; that is, if we understand him right, the supposition that there is in the process of second dentition something which is attended with the simultaneous absorption of the milk teeth. This, it may be remarked, is hardly entitled to the character of an explanation, and is rather a different mode of stating the facts. That the two actions of formation of the permanent and absorption of the temporary teeth are simultaneous, is matter of fact. The point to be ascertained is what renders them simultaneous, or what is the general fact to which both are referable? So far as we are aware, the only rational explanation is that which ascribes both actions to the transference of the vessels of the temporary to the pulp of the permanent teeth. "The moment this process (that of the formation of the permanent teeth) commences, a new train of phenomena takes place with the primary teeth. The follicular sacs of the new or permanent teeth are liberally supplied with vessels for the purpose of nutrition; and as these blood-vessels increase in size, those of the temporary teeth diminish; and the supply of blood being thus cut off, the latter undergo a sort of natural death. The roots, which as being last formed, are not unfrequently incomplete, now undergo a process of absorption, and the tooth drops out in consequence of the destruction of its nutritious vessels. Some authors have ascribed this expulsion to pressure, exercised by the new tooth. They forget, however, that before the new tooth can exert any pressure, it must be in some degreee formed, and to this a vascular system is indispensable."*

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* Elementa Physiologiae, Tom. vi.

On the interesting subject of *Irregularity* are found some judicious observations. Referable to adult teeth only Mr Bell considers it as of two kinds, temporary and permanent. Of the temporary irregularity there are two varieties, the first depending on want of concert between the absorption of the temporary, and the advance of the permanent teeth, which are thereby forced through the gum, either before or behind the former; the second from difference in size between the temporary and permanent *incisores* and *cuspidati*. Permanent irregularity is the more serious deformity which results from disproportion between the extent of the maxillary arch and the size of the permanent teeth, the maxillary bone not extending so rapidly, or in the same degree in which the permanent teeth are developed. Either of the varieties of temporary irregularity, if not obviated by the seasonable removal of the temporary teeth, may become permanent. Permanent irregularity is also the occasional result of malformation of the jaw, or the appearance of supernumerary teeth.

No subject has given occasion to so much officious interference on the part of the dentist as the prevention of irregularity; and upon no one perhaps have so much ignorance, misconception, and even charlatanism prevailed. These evils Mr Bell is willing to ascribe to the example of Mr Fox, and to the precepts inculcated by that author. This we believe is not alto-

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[*] Elements of General Anatomy, by D. Craigie, M. D. P. 550. Edin. 1828.

gether groundless; but we must also observe, that a good deal of the ignorance and charlatanism have prevailed also among dentists, who were totally unaware of the instructions of Mr Fox. Not a little also of the cruelty as well as the folly which have been practised in this matter, must be set down to the account of the absurd anxiety and foolish importunity of parents and guardians, who, the moment they think the milk teeth of a child are rather slow in being shed, carry the unhappy subject to the dentist, who, unless a person of integrity as well as of knowledge, is not always scrupulous about complying with the wishes of the anxious parent. On this topic it is justly remarked by Mr Bell, that "the legitimate object of the surgeon is confined to the application of remedies in disease, or the regulation of the natural functions when deranged; and it is not to be supposed that a process of so much consequence to the comfort and health, and essentially connected with a function so important as that of digestion, should be so imperfectly provided for as to be constantly in need of such harsh and unnatural interference. I have known no less than eight and even ten firm teeth forcibly removed from the jaws of a child at once."

The practice of forcibly extracting the milk teeth by anticipation to prevent irregularity of the permanent teeth, Mr Bell condemns in the most unqualified terms, as equally cruel, unnecessary, and improper, for two reasons. The first of these is the connection which subsists between the temporary tooth and the succeeding permanent one, by means of the vascular cord extending from the sac of the latter to the neck of the former, and which is necessarily rent asunder by this proceeding to the great danger of interfering with the healthy and regular formation of the permanent tooth. The second is the fact, that the premature removal of the milk teeth before the permanent ones are ready to fill their spaces, allows the jaw to contract so much, that it may be greatly too small for containing the permanent teeth, and thus precipitates the approach of the very evil which it is intended to obviate.

It is almost superfluous to say, that no attempt ought to be made to interfere with the progress of nature in the developement of the permanent teeth before they appear through the gum, and so long as the temporary ones are firm in the jaw. The ultimate regularity of the former may be inferred from the expanded and semicircular shape of the maxillary arch, from the temporary teeth being slightly separate or at least not crowded, from the permanent *molares* being well-shaped, and of moderate size, from the absence of prominence behind the temporary teeth, and from the parents and other members of the same family having broad well-shaped arches and regular teeth. For general rules Mr Bell regards the following as sufficient to regulate the management of the process of permanent dentition. If the inferior permanent central incisors appear through the gum behind the temporary, the latter, though not loosened, may be removed; yet in the author's experience no injury has been found to result from leaving them till the permanent teeth acquire some size, unless when the jaw is badly formed. If the removal of the two central incisors be insufficient to allow the others to come forward, it is expedient not immediately to remove the temporary lateral incisors till the permanent lateral are ready to pierce the gum; and even if these should require additional space, the temporary *cuspidati*, which are important in preserving the natural shape of the jaw, should be retained as long as possible.

In regulating the dentition of the upper jaw, Mr Bell recommends more frequent inspection, and sometimes earlier removal of the temporary teeth, for several reasons. Since the teeth of the upper jaw pass before those of the lower when the mouth is shut, if the permanent incisors appear behind the temporary and advance so far, previous to the removal of the latter, as to fall behind the lower incisors in shutting the jaws, the latter will form a permanent obstacle to their advancing to the natural position. This circumstance, with the comparative magnitude of the superior incisors, renders early removal of both the central and lateral ones a measure of necessity. Upon the same grounds, Mr Bell sanctions the removal of the superior temporary *cuspidati*, when the permanent lateral incisors are appearing behind them, if the removal of the temporary lateral incisors fails to afford an adequate space for their growth.

Permanent irregularity, which, in the majority of instances, depends on the disproportionate small extent of the maxillary arch to the teeth, is not so easily remedied. The first measure recommended by Mr Bell, in cases of slight permanent irregularity, is to remove a portion from the lateral surfaces of several of the teeth by means of a thin file passed between them, in consequence of which considerable space is procured, to allow the irregular tooth to be brought into its place by moderate pressure. To the extraction of any one of the permanent teeth he is decidedly hostile, unless when no other means seem adequate to the object; and he particularly impresses the necessity of waiting a few years till, by the gradual enlargement of the jaw, it be ascertained, that the maxillary arch is to acquire sufficient extent to contain the teeth; and it is some encouragement to know, that in his experience he has seen this completely effected at the age of 14 or 15 years. If, however, after giving this term of trial, the maxillary arch seems still to be small and contracted, it becomes requisite to sacrifice one of the permanent teeth. Previous to the time of Mr Fox, the practice was to remove the irregular tooth; but, as the *cuspidati* or canine teeth are most usually the irregulars, and as these and the incisors are much more important than the bicuspid, Mr Bell thinks it much better, in ordinary cases, to remove one of the latter. If a permanent molar tooth is affected with decay, he recommends its removal as a general rule, to allow the *biscuspides* to fall back and give space for the others to come into their natural situation. He is in all cases averse to the sacrifice of a central incisor; and he recommends, in preference, when indispensable, the loss of a bicuspid or lateral incisor.

In attempting to correct irregularity in the teeth of the upper jaw, Mr Bell approves of the method practised by Mr Fox, so far as to adopt it with some modification. The plan of the latter author consisted in the application of a bar of gold to the dental arch, and secured by silk ligatures to the temporary *molares*, with a ligature passed through it round the irregular tooth, which was thus to be gradually but forcibly drawn forwards. On this method Mr Bell makes the following improvements. The gold bar is accurately stamped to a brass cast or mould of the teeth, so as to fit exactly all the natural inequalities of the dental arch, and thereby to be fixed more firmly and with greater ease and comfort to the patient. Instead of the square flat piece of ivory interposed between the *molares* of the upper and lower jaws, to prevent the lower teeth from closing in front of the upper ones, Mr Bell uses a single cap of gold accurately applied to a molar tooth of the upper or lower jaw; the former most eligible. This contrivance is represented to be highly efficient, and applicable to almost all the varieties of irregularity. When the tooth is so far brought forward that its edge is in the smallest degree anterior to that of the lower teeth, the *gold cap* is recommended to be removed, to allow the pressure of the latter teeth behind the irregular one to complete the cure, to which, in general, it will be quite adequate. The *bar* is left a few days longer, to prevent the tooth from returning to its unnatural position.

Supernumerary teeth he recommends to be extracted wherever they can be distinguished, which, however, is not at all times easily done. The instrument most suitable for this purpose are straight extracting forceps, with the beaks as slender as consists with the requisite degree of strength, and with only a slight degree of curvature.

In some rare instances two or more teeth become united by their roots into a single bony mass. Examples of this malformation are preserved in the Museum of Guy's Hospital, and Mr Bell's experience has supplied him with four instances, in three of which the temporary superior central incisors were thus consolidated, so that it is impossible to deny its occurrence, as has been attempted by some writers. On the mode in which it takes place, the author gives little information; but it appears to be his opinion that in general the connection is effected during the process of ossification, probably by the early union of two pulps. Any cause, indeed, which produces this union, either by rupture or laceration of their contiguous parietes, or by absorption before the alveolar partitions are fully completed, would have the effect of consolidating the roots of two or more teeth. The evil, bad as it is, is unknown till the attempt to extract one of such teeth either brings out both, or rends the one from the other by fracture.

In the second part of the work, on the Diseases of the Teeth, Mr Bell, after some observations on the difficulties of selecting a suitable nomenclature, avows his determination to abide as far as possible by the terms already in use, and to depart from them only where they imply most erroneous notions, or palpable absurdity.

The first affection which comes under notice furnishes an example of the latter alternative. This is what is commonly, but erroneously, according to Mr Bell, called *caries*, and which he therefore designates gangrene, and defines as *mortification of any part of a tooth, producing gradual decomposition of its substance.* The accuracy of this definition it is not our intention to dispute. It is sufficient to say, that, whatever difference of opinion it may give rise to, the accuracy of the subsequent description is completely calculated to remove. Attacking usually the crown, more rarely the neck, and almost never the root, Mr Bell represents it as appearing first on the external surface of the bone, immediately beneath the enamel, and betraying its existence by an opaque spot seen through the translucent enamel. The first effect of this change is disintegration of the crystalline texture of the enamel, which then cracks, and drops off in fragments, until the diseased portion of the tooth is exposed in the form of a dark-coloured, irregular excavation. The progress of the disorder to the internal cavity causes inflammation of the membrane,—the pathological state which gives rise to toothach. In those instances in which the decay is so gradual that the membrane is not soon exposed, it is absorbed before the decay reaches the cavity, and the tooth breaks down without ever occasioning pain.

In explaining the etiology of this affection, Mr Bell equally rejects the speculations of Mr Hunter and the doctrine of Mr Fox. Inferring from the organic structure of the osseous portion, that it is liable, from the operation of the ordinary causes, to inflammation; and from the appearances of teeth extracted during the presence of toothach, that the process actually does take place, he thinks that this is the genuine proximate cause of gangrene of the teeth. This action, he argues, impairs the vitality of the bony portion, which then is unable to retain its vital powers, and mortification is the result. A portion of tooth once killed in this manner operates on the contiguous bony structure, as in other textures, by exciting increased vascular action, which in its turn also terminates in gangrene; and so forth, at variable rates of rapidity, till the whole crown of the tooth is destroyed. This view is, on the whole, plausible and ingenious; but we do not perceive that it excludes the opinion of Mr Fox, who maintained, that in all instances the inflammation originates in the pulp or internal membrane, the effect of which is to kill the vessels of part of the tooth, and consequently that portion of the tooth also. Strictly speaking, it may be doubted, although the bony portion is proved to receive vessels, whether the particles of bone are susceptible of inflammation. These separate atoms are inorganic, and though nourished by vessels which pass between their interstices, it by no means follows that they are themselves living. Inflammation of the pulp and spreading along their vessels would give all the appearances recorded by Mr Bell, and produce all the effects which he ascribes to inflammation of the osseous portion of the tooth.

Among the remote causes of this affection Mr Bell assigns a conspicuous place to mercury, the use of which in early infancy, he maintains, produces that general tendency to decay which in many instances destroys almost a whole set of teeth at an early age. Among exciting causes sudden transitions of temperature, the use of substances very hot or very cold, and the mutual pressure of teeth too closely set in a small maxillary arch, are also enumerated as important. The operation of contact he denies altogether, and ascribes to the circumstance last mentioned the effects attributed to this imaginary cause.

The treatment he divides into prophylactic and curative. To prevent the first indication he recommends, that whatever fluids are taken into the mouth should neither be so hot or so cold as produce pain or any disagreeable impression, and that water employed for cleansing the mouth should, during winter at least, be tepid. The utility of this rule can be estimated only by actual experiment; and in no case in which we have seen it used has it failed to produce the greatest benefit, at least in rendering fits of toothach milder and less frequent, and altogether rendering the teeth less liable to disease. Bleeding from the gums by leeches is recommended during the actual presence of pain. When the teeth are too closely set, so as to rub on each other, a little should be removed by the file from their sides, with the precaution, however, of not going completely through the enamel, and exposing the bone, which would have the effect of precipitating the evil. In the case of a bicuspid or molar tooth already considerably diseased, its removal becomes a measure of expediency; and in some instances the adjoining teeth, by tending towards the vacancy, take off pressure and supersede the necessity of the file.

In the curative treatment, the proceeding recommended by Mr Bell is, on the first indication of decay to remove by incision the diseased spot. As this, however, is merely the substitution of a less for a greater evil, he censures the use of the file, and recommends that the portion should be cut away by strong three-sided steel brooches, and afterwards smoothed by the use of appropriate instruments. When the diseased portion is not superficial but penetrating, it is requisite after removal to fill the cavity, or plug it by means of some substance not liable to chemical change; and for this purpose gold is preferred by the author. In giving the details of this procedure we are happy to perceive that Mr Bell limits the dentist to a few well-contrived instruments, and is particularly averse to that instrumental parade which several operators seem to think quite indispensable to success. Mr Bell, on the contrary, divesting his practice of all pedantry and refinement, is desirous to impress on his readers the important truth, that the excellence of the surgeon is by no means in the direct ratio of the multiplicity of instruments which he employs for operation, but must be estimated rather by the ingenuity with which he can employ a few good instruments to a great variety of different purposes. As a general rule, he recommends straight or slightly curved instruments for the upper teeth, and for the lower, instruments bent at right angles. "It is not with any invidious feeling or object that I here advert to the employment of not fewer than 120 instruments in stopping the teeth, accompanied by the assurance that even this collection will not in some cases furnish the requisite form; but I think it necessary to remove from the mind of the young practitioner, some of the alarm and despair which must naturally seize him on contemplating the mysterious and interminable nature of his acquirements when entering on the practice of his profession. I do not hesitate to say that, to a person of common manual dexterity and a little mechanical contrivance, the instruments I have described will be sufficient for all common cases, and certainly all that the general practitioner in the country will be ever likely to require; whilst, to the professed dentist, it will never be difficult to modify or construct *extempore*, such additional instruments as are called for in anomalous cases."

In a third degree, when the bone has become softened to such extent as to occasion pain on pressure, the removal of the part exposes the membrane, and renders it impossible to plug the tooth without inducing severe pain and inflammation, and if not removed, the introduction of any substance on the softened bone has equally pernicious effects. It is singular that any dentist should be found rash enough to attempt the operation under these circumstances, since the action is so violent as in some cases to resemble closely *tic douloureux*, and in others, by spreading to the periosteum, and socket to terminate after long and excruciating agony in suppuration. Yet that this is actually done is established by cases alluded to by Mr Bell, and two of which he details. To relieve these symptoms, the obvious course is to withdraw the stopping, to subdue inflammation by the use of leeches, and some soothing remedy to the exposed membrane. The best application of this nature, Mr Bell finds to be a solution of nitrate of silver, introduced by a camel's hair pencil, or on a slip of charpee, after the cavity has been cleansed and dried.

> " As a general rule," concludes Mr Bell, " and one to which I would allow no exception, a tooth should never be filled at a time when the pressure of the gold occasions that peculiar pain which arises from contact with the membrane. It does not, however, necessarily follow, that because the interior of the tooth is tender, or even the membrane exposed in a slight degree, no hope is to be entertained of filling it at a future time. If the cavity be of a size and shape favourable for the operation, and the portion of the membrane exposed be trifling, it often happens that measures may be successfully adopted, either to occasion the gradual absorption of the part, or at least such a diminution of its sensibility as to allow of its being filled."

With the latter view, Mr Bell proposes to destroy the nerve, or rather he ought to say the internal membrane, by the actual cautery at a white heat, or to diminish its sensibility by the use of opium, or preferably camphorated spirit, or solution of nitrate of silver.

On the trite subject of toothach, the only thing we notice is, that Mr Bell recommends for alleviating the pain of that excruciating complaint the local application by means of lint, of a solution of alum ʒi., in sweet spirits of nitre ℥ss., or muriatic acid, ʒss, in distilled water ʒii., or nitrate of silver gr. i. in the same vehicle, ʒi.

Gangrene of the temporary teeth, Mr Bell recommends to be treated on much the same principles as that affection in the permanent set.

Necrosis, Mr Bell defines to be that condition of a tooth in which the vitality of its whole substance is at once destroyed, when it presents none of the appearances which distinguish partial and progressive gangrene. This state may be induced by any cause operating with sufficient force and suddenness on the internal membrane and nutrient vessels; for instance, external violence, inflammation, fever, the immoderate use of mercury, &c. Though it may attack any one of the teeth, the *incisores* and *cuspidati* are most frequently affected. This change gives the tooth a dark appearance, which deepens until it becomes almost black. As the tooth is truly a foreign body, inflammation is set up round it, the gum becomes swelled and spongy, and matter, the result of suppuration of the socket, is discharged, either at the margin of the gum, or through one or more openings opposite the root. The alveolar processes are absorbed, and the tooth is either loosened and expelled, if not previously removed by art, or lies horizontally and quite useless on the gum.

For this disease Mr Bell recommends free and repeated scarification in the early stage, followed by the use of astringent lotions or injections, of which sulphate of zinc is most commended. Afterwards, however, as the tooth is a foreign body, it ought to be eradicated.

Of the same nature nearly, Mr Bell regards the results of those cases in which, with the view of curing toothach, a tooth has been extracted, and after excision of the decayed part and stopping with gold, replaced in the socket. This method, which was proposed by Mr Fox, was found, however, to fail in answering expectation, by reason of the appearance of all the symptoms indicating death of the tooth. The mode in which this takes place is easily understood when the effect of extraction on the nutrient membrane is remembered.

Transplantation, which is also considered in this chapter, the author justly condemns; and, indeed, it is impossible to think of the effects which this not very civilized practice produced in the greater number of instances in which it was used, without regarding it as the most singular specimen of barbarous and irrational treatment that was ever attempted to be introduced into the practice of surgery. From this the name of Hunter, great as it is, derives no additional lustre.

Under the head of *Abscess of the Bony Structure*, which is doubtless an extremely rare disease, Mr Bell records an instance in which, upon extracting and dividing by the saw the second molar tooth of the lower jaw, a circumscribed cavity was found in the solid bony texture, the surrounding bone being white and healthy. Mr Bell, however, does not state as part of the case, that the cavity *actually contained* purulent matter, but only that "the vessels of the bone had formed pus, and that absorption had taken place in consequence of its pressure, and formed a cavity for its reception."

Exostosis of the teeth, which consists in irregular deposition of hard dense yellowish matter, slightly transparent, and remotely resembling calcedony, Mr Bell ascribes to increased irregular action of the vessels, a variety of slow chronic inflammation, the result of incipient gangrene, or any similar cause of moderate but continued irritation.

On the subject of mechanical injuries, and their mode of treatment, are given some good observations, into which, however, we cannot more fully enter.

Abrasion, or *detrition* of the enamel, commonly called wearing down of the teeth, is said to consist of a gradual loss of substance, generally without discoloration or any diseased appearance,—a mode of definition which cannot be said to illustrate the nature of this obscure affection. The supposition of Hunter that it is an original disease of the tooth itself, independent of accident, way of life, constitution, or any particular management of the teeth, might be probable if the change was in all cases confined to one or two teeth only. In a large proportion of instances, however, it affects the crowns of a whole arch, and sometimes of both arches. Fox, again, remarking that it "is confined to that portion of the teeth which could not be acted on by the friction of one tooth against another;" that it occurs "both in the healthy and the delicate;" and that "it appears to be connected with some cause which may produce solution of the enamel, thinks it very possible that the saliva may have some influence, and that the friction of the lips may contribute to the removal of the enamel." The first of these two causes Mr Bell thinks wholly at variance with the fact that certain teeth are more liable to it than others. Its invariable commencement on the enamel, he further remarks, is inconsistent with the belief that it is produced by absorption; yet, while he admits that the most obvious solution of the difficulty is, that it is the result of friction, he thinks he has seen it so often aggravated if not produced, by rough or acidulated tooth powders, that his only hesitation to ascribe it to such a cause, is, that in other cases the detrition has taken place to a considerable extent, where no applications of this nature had ever been employed, and he concludes with avowing his inability to explain the phenomenon.

Amidst this discordance of evidence and reasoning, it is by no means easy to come to any satisfactory results. We take this opportunity, nevertheless, to remark, that in some instances it may be traced partly to friction, aided by a softening property of the saliva, and in others it is attributable very much, if not entirely, to an acidulous state of the fluids of the mouth and stomach, connected in all probability with impaired digestion. Thus the lower incisors of many persons, from constantly rubbing against the posterior surface of the upper incisors, undergo detrition of their apices with an oblique surface, corresponding to that against which they rub. In such persons the bicuspid and molar teeth are generally unaffected. Again, we have seen, in persons liable to dsypeptic disorders, and in one especially, a female who had frequent fits of sick-stomach, terminating in vomiting, the crowns of the whole arch worn down to such an extent as to expose the interior bony pillar, surrounded by its ring or hoop of enamel. In these instances it is surely unnecessary to look for any other cause than the softening property of the gastric and salivary fluids; in consequence of which the ordinary friction operates with much greater effect than in healthy persons. There is, however, another class of facts which must not be neglected in this investigation. Among certain savage tribes the crowns of the teeth are, in process of time, worn down to the extent of a line or more, so as to expose most completely the internal osseous part of the tooth. A professional gentleman of this city has in his possession the skull of one of the chiefs of the native tribes of New Holland; and though it is preserved chiefly as a specimen of the irregularities produced on the skull by the blows inflicted in their hostile encounters, and of the frequent injuries which are inflicted without proving fatal, it is not less remarkable for exhibiting a most perfect and advanced example of detrition of the teeth, confined not to one or two, but extending over the whole arch. Among these tribes this detrition is not uncommon; and that it cannot be ascribed to mere friction is certain, from the fact, that they do not restrict themselves to food made from grains and similar substances requiring much mechanical division. The last fact which we shall mention on this head is, that in a cast of the skull of an orang-outang preserved in the museum of the Royal Society of this place, and the original of which is in the cabinet of the Medico-Physical Society of Calcutta, the teeth are represented as worn down to an extraordinary degree.

On the subject of *diseases of the internal membrane*, Mr Bell rejects the analogy on which Mr Fox denominates its inflammation *spina ventosa* of the tooth. The analogy is certainly not perfect, no more than a bone is a tooth, or the medullary membrane of the former the nutrient pulp of the latter. In so far, however, as it is a mere name, little harm can result from its use.

Under the head of *Diseases of the Gums and Alveolar Processes*, Mr Bell condemns the use of the term *Gum-boil* as highly erroneous, and proposes to substitute that of alveolar abscess, which he conceives communicates a more correct idea of the nature of the disease; since, as he argues, the gum is only secondarily affected, and the primary malady is seated within the *alveolus*. Without disputing the propriety of the term *alveolar abscess*, we must be allowed to say, that every case of inflammation and suppuration of the gum is not necessarily connected with disease within the alveolus; and though Mr Bell may find that many of them are attributable to this cause, some are confined entirely to the soft texture of the gum. From the description given, indeed, by Mr Bell, we are disposed to regard the disease by him named alveolar abscess as totally different from that of gum-boil, which is neither more nor less than phlegmon of the cellular tissue of the gums, the *parulis* of practical authors. Mr Bell's description is very excellent, and should be studied by all who wish to know the characters of genuine alveolar inflammation. The treatment consists in the use of local antiphlogistics; and afterwards, if these fail to effect resolution, the purulent matter must be evacuated by incision. In the ordinary gum-boil, the best treatment consists in the local detraction of blood by leeches, free division of the gum, or incision, so as to unload vessels and relieve tension, or evacuate blood and discharge matter.

Mr Bell then enters on the consideration of *tumours rising from the gum and alveolar processes*, of which he gives several instructive examples; *displacement of teeth by deposit of bone in the alveolar cavity; the effects of mercury on the teeth, gums, and alveoli;* and finally diseases of the maxillary sinus. In the discussion of the latter subject, which approaches closely on the confines of surgery in general, Mr Bell shows much judgment and observation.

On the subject of *extraction of the teeth*, an operation so commonly practised, so much abused, and which forms such a fertile subject for criticism, we do not propose here to enter; and we refer our readers to the section in the present volume, where they will find the subject treated in a scientific manner. The author distinguishes carefully the modes of operating adapted to each kind of tooth, and to the accidental peculiarities induced by disease or malformation. To the practice of dividing the gum previously he is averse, though we cannot perceive for any good reason. That it does not facilitate the removal of the tooth from the socket is not the point at issue in the question; for if the tooth is jerked out without this preliminary operation, the gum is torn, and it is often requisite after all to use the knife in completing the division. Of the tooth key he seems to make little use, employing it chiefly for the removal of the bicuspides and molars of the lower jaw, and the third and fourth molars of the upper. The great substitutes are Hawksbill forceps, of large and small size, which he employs to remove not only the *incisores* and *cuspidati*, but the *bicuspides*, molar, and *dentes sapientiæ* of the upper jaw, and the bicuspides, first and second molar, and *dentes sapientiæ* of the lower. On this point Mr Bell's experience must be allowed to have great weight. But we cannot refrain from observing, that no forceps can be used with the steady and sure action which can at all times be obtained by the lever and claw of the key instrument. It is no argument against the use of an instrument that it is occasionally abused; and we suspect after all that the key is safer in general hands than any other species of extractor. If, however, Mr Bell really can simplify the instrumental cabinet of his brethren, he deserves the thanks of the profession; and ours, we assure him, shall not be withheld.

His adaptation of a rack to the Hawksbill forceps seems ingenious, and we daresay will answer expectation. But on this point we observe that the relinquishment of the key is the very circumstance which has rendered this complicated contrivance requisite. It is the advantage of the key that when once fixed, nothing can make it slip, unless the tooth be too small; and every turn that it makes causes it to grasp the tooth more firmly, till the latter is started from the alveolar cavity.

The volume is concluded with observations on neuralgia, constitutional and local, upon which our limits do not permit us to enter. But we have much pleasure in recommending them to the notice of our readers, who will find that Mr Bell makes some distinctions which are of great practical importance in the treatment of the disease, and the adaptation of remedies to its several varieties.

ART. III.—1. *Some Account of a Case in which the Uterus, in a state of malignant ulceration, was successfully removed.* By JAMES BLUNDELL, M. D. Lecturer on Physiology and Midwifery in Guy's Hospital, London. (London Medical Gazette, August 1828.)

2. *Case of Extirpation of the Uterus.* By JOHN M. BANNER, Surgeon to the North Dispensary, Liverpool. (Ibid. October 1828.)

3. *Two Cases in which Extirpation of the Uterus was performed.* By JAMES BLUNDELL, M. D. &c. (Ibid. November 1828.)

4. *Observation de l'Extirpation complete de l'Utérus pratiquée à l'Hotel-Dieu de Paris.* Par M. le PROFESSEUR RECAMIER. (Arch. Gén. de Médecine, 1829, xxi. 78.)

5. *Deux cas de l'Extirpation de l'Utérus suivis de mort.* Par M. Roux. (Bulletins des Sciénces Medicales, Octobre 1829.

Since describing *Dr Sauter's* case of extirpation of the uterus in our number for July 1824, we have not hitherto felt it incumbent on us to take farther notice of the operation, or of the various attempts which have been made in this country and elsewhere to follow the example of that surgeon and his adventurous countrymen. An operation so dazzling in its circumstances, and so tempting in its object, could not fail to find many men bold enough to incur the responsibility of its manifold risks. And accordingly in the course of the last two years no fewer than nine cases have been published in which it was attempted. These we have not noticed as they occurred, because we scarcely consider it a legitimate part of the duty of the Quarterly Reviewer to review individual cases, which singly admit of no general conclusions. By waiting till cases have accumulated in sufficient number, it is at length in our power to present the reader with general results, which to the greater part of the surgical profession are of much more interest than the particular details of the operations. For, with respect to an operation of this kind, the question which the surgeon will have to consider is not so much, whether he should perform it, as whether he would advise it,—not one perhaps being to be found in a hundred who would encounter personally its difficulties and dangers.

Under these impressions we proceed to give an abstract of the instances in which the entire uterus has been removed; and at the close some general observations will be made on the probability of this operation becoming a regular and legitimate part of operative surgery.

The first idea of the possibility of removing the diseased uterus from its position in the pelvis was evidently taken, as appears in our review of *Dr Sauter's* work, from first the accidental, and subsequently the intentional removal of that organ when in a state of prolapsus or inversion. Sometimes it was removed accidentally with the knife by ignorant persons, who mistook it for a polypus or other tumour; at other times it was purposely removed by scientific surgeons with the ligature, as the only effectual remedy for a troublesome, loathsome, and dangerous disease. This is a very different operation from the one which is the proper subject of the present article. But as it has been of so much importance in giving rise to the operation of removal of the uterus from its natural situation in the pelvis, it may be well to consider what additional facts have lately appeared on the preliminary question of the propriety of removal of the prolapsed or inverted uterus.

In addition to the cases alluded to in our review of *Dr Sauter's* treatise, several instances have recently occurred in this country, where the whole or a great part of the uterus has been removed when it was protruded in a state either of inversion or of prolapsus. In 1818 *Mr Newnham* published an account of the extirpation of the uterus in a case of inversion.* In 1819 *Mr Windsor* of Manchester published in the Medico-Chirurgical Transactions † an account of a similar instance, where he removed a large portion of the fundus of the uterus, together with the greater part of the Fallopian tubes. In 1822, *Dr Charles Johnson* described in the Dublin Hospital Reports ‡ two similar cases, in each of which the fundus and Fallopian tubes were removed. In the Medical Gazette for June 1828, a fifth case of the kind has been briefly noticed. The most striking instance of the present description, however, is one related by *Professor Récamier* § in the Archives Générales de Médecine for January 1826.

In this case the patient, after being long liable to prolapsus of the uterus, was attacked about the age of 50 with a muco-sanguinolent discharge from the vagina, which was traced to a fungoid tumour of a cancerous nature formed on the cervix of the prolapsed uterus. When the woman was reduced to such a state that death at no very remote period appeared inevitable, *M. Récamier* resolved to remove the tumour and prolapsed uterus by a ligature. This he was encouraged to attempt on finding, that the finger placed in the rectum met the hand on the hypogastrium, without encountering any organ or tumour. Accordingly, on pulling the tumour gently down to the vulva, a curved needle was easily thrust through the vagina above the fundus of the uterus, and being provided with a double ligature, this was drawn through and tied on each side. After fifteen days, and repeated tightening of the ligature during that period, the mass thus isolated became gangrenous, and was removed with the knife on account of the excoriation caused by the acrid discharge. It was distinctly ascertained that the whole uterus had been removed. The patient did well, and the wound soon healed up.

In all of these cases the operation was performed on account of the exhaustion produced by a constant muco-sanguinolent discharge, which no ordinary means could arrest, and which threatened the speedy extinction of life. In all, the tightening of the ligature, especially at first, was attended with pain in the pelvis, or also in the abdomen, back, and thighs, and likewise with sickness, vomiting, and some general fever. These symptoms frequently approached so nearly the characters of peritonæal inflammation, that it was considered necessary to relax the ligatures again and again. But, nevertheless, in every instance the threatening symptoms were but temporary, and all the patients eventually recovered. It is, therefore, highly probable, that in bad cases of inversion or prolapsus uteri, where reduction is impossible and life in danger from the excessive discharge, the removal of the whole or a part of the uterus by ligature is not a very dangerous operation, and may be practised with the strong chance of a permanent and speedy cure.

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* Essay on Inversio Uteri, 1818.

† Medico-Chirurgical Transactions, x.

‡ Dublin Hospital Reports, iii.

§ Archives Générales de Méd. x.

The operation of excision of the uterus from its natural situation, however, although in many respects not materially different from that we have hitherto been considering, appears to be far more hazardous and of much more doubtful value. The following is a detailed abstract of the principal cases which have come under our notice.

I. The first we shall mention is one of *Dr Blundell's*, who has been particularly active in this department of operative surgery, having removed the scirrhous uterus no less than four times. A female 50 years of age had suffered for some time under carcinoma of the uterus, which affected also the vagina, and had advanced to the stage of ulceration. The uterus was of the size of a goose's egg; but so far as could be judged, the other organs of the pelvis, as well as the lymphatic glands of the groin, were not diseased. She had long laboured under profuse bloody discharge, by which her strength was much exhausted, but nevertheless she appeared to have vigour enough remaining to justify the hope that she could stand a formidable operation. The operation of excision was therefore resolved on, and performed in the following manner.

Dr Blundell divides it into four stages. *First*, An incision was made through the posterior surface of the vagina. This was done by applying the first and second fingers of the left hand at the point of junction between the healthy and diseased parts of the vagina, and placing under the protection of the point of the fore-finger, a long-handled scalpel, the extremity of the plane of which made an angle of 15 or 20 degrees with the axis of the handle. In this manner the point was cautiously urged through the vaginal membrane, without wounding the fore part of the rectum,—care being taken to withdraw the cutting edge occasionally within the shelter of the finger, and to examine the wound with the nail. *Second.* The incision now made was extended right and left to the root of each broad ligament. For this purpose the opening was first enlarged by dilating and tearing it with the fore-finger; and then, the point of the knife being withdrawn within the finger, and its edge protruded slightly beyond the side of the finger, the incision was first extended towards the left. The extension towards the right was also effected by substituting for the knife hitherto used another of similar construction, but with the cutting edge on its opposite side. The whole posterior surface of the vagina was now divided, and the tips of the fingers were in contact with the intestines. *Third.* The uterus was next retroverted and pulled with the fundus foremost into the vagina. This was accomplished by thrusting the whole hand into the vagina, and the fingers through the incision in its posterior surface along the back of the uterus,—introducing a long-handled double hook under the protection of the fingers into the same situation,—pressing the hooks with the fingers into the uterus,—drawing the uterus gradually downward and backward, while the fingers were passed onward over the fundus,—and finally retroverting the uterus till it lay in the palm of the hand, by this time again in the vagina. The diseased organ could then be seen at the vulva. *Fourth.* It now only remained to divide the broad ligaments and Fallopian tubes, the peritonæal connexion of the uterus with the bladder,—the fore-part of the vagina—and the connecting cellular tissue between the uterus and bladder. In these steps, care was taken to divide the broad ligaments close to the uterus, and to keep clear of the neck of the bladder and ureters while dividing the vaginal membrane.

The operation was thus concluded in about an hour without the loss of more than four or five ounces of blood, and it was not necessary to secure any bleeding vessel. The pain caused was inconsiderable, except when the uterus was retroverted, and the ligaments stretched by pulling it downwards.

The woman's recovery was easy; but the particulars have not been made public. Five months after the operation she was fat and well, and the head of the vagina was closed up by the bladder. Her sexual desire continued strong. About a year after the operation she became a patient of *Dr Bright* in Guy's Hospital, for obstinate constipation of some weeks standing. No stricture could be discovered in the gut, and injections could be thrown up. But nevertheless the constipation increased, was complete for five days before her death, and was accompanied with much distension, but not with any pain of the belly. She expired with all the symptoms of obstructed intestine.

On dissection it was found that, about four inches from the anus, the rectum was considerably contracted, and at one point so much as to prevent the passage of a large pair of scissors. Its course was also unnaturally tortuous. The colon and lower part of the small intestines were much distended, and peritonæal redness and effusion were found on their surface. The vacuity formed by the removal of the uterus appeared to be covered over by the bladder, the ovaries, and the divided ends of the broad ligaments. In various parts of the pelvis cerebriform and carcinomatous tumours were found. Thus small scirrhous tubercles lay under the peritonæum near the site of the uterus, some of them under the peritonæal coat of the bladder; cerebriform tubercles were formed beneath the mucous membrane of the rectum where the stricure was; both ovaries were considerably altered by fungoid degeneration; the fat of the pelvis was firm, and here and there interspersed with small scirrhous tubercles, and a mass having the characters of true scirrhus extended upwards as far as the iliac vessels; the vagina, though for the most part healthy, presented at its upper extremity an ulcerated surface, connected with a soft cerebriform mass of the size of a walnut, and projecting upwards into the gape formed by the removal of the uterus.—[*Lond. Med. Gazette*, ii. 294, and iii. 797.]

II. The next case we shall mention occurred at Liverpool in September 1828, the operator being *Mr Banner*, surgeon of the North Dispensary there. The subject of this case, a female 44 years of age, was first seen by her surgeon sixteen months before the operation was performed; and at that time there was thickening, hardness, and irregularity of the neck of the uterus, for which it was proposed to perform amputation of that part only of the organ. To this proposal, however, the patient would not consent. By and bye a bloody sanious discharge took place from the vagina, her strength became much exhausted, and in September 1828, *Mr Banner* found the scirrhous state of the uterus had extended farther on the body of the organ than he could reach, and that the *os tincæ* was ulcerated. Extirpation was therefore proposed on the ground of *Dr Blundell's* success in the former case, and she resolved to take the chance of it.

Mr Banner followed a different plan from that pursued by *Dr Blundell.* He fixed a hook in the fore-part of the neck of the uterus, pulled the uterus down into the vagina, and secured it near the external opening of the vagina by passing a ligature through it, which was entrusted to an assistant. The vagina and peritonæum were then divided, first before and then behind, with a common scalpel upon the lower part of the neck of the uterus. Next the fore and middle fingers of the left hand were passed, one through the posterior the other through the anterior incision, with the right broad ligament between them; which was then partially divided close to the uterus. An attempt was made to check the hemorrhage which ensued by securing the bleeding vessel of the ligament; but it was unsuccessful and proved unnecessary. The operator here abandoned his intended method as tedious and difficult; and adopted a modification of *Dr Blundell's* mode of turning out the uterus. It was accomplished with the fingers and hook as in the former case, introduced, however, at the anterior opening. The uterus being turned forward, the broad ligaments and Fallopian tubes were seen and easily isolated with the finger; upon which they were divided close to the uterus. The operation was finished in twenty-five minutes, and might have occupied a much shorter time, had it not been for the unnecessary attempt to secure the divided vessel of the right broad ligament. About six ounces of blood were lost. The intestines did not protrude or otherwise interfere with any part of the operation.

Soon after the operation two clots of blood, each amounting to eight ounces, were discharged; but this was the whole amount of the hemorrhage. She had also several attacks of fainting, with pain in the belly and constant vomiting, which were relieved from time to time for a few hours by opium. She passed a restless night, with some tension of the belly from retention of urine; but was relieved after the use of the catheter, and on the evening of the second day appears to have rallied considerably. On the morning of the third day the belly was tender on pressure, and became more and more so towards evening, so that first leeches, and then venesection were resorted to. The most distressing complaint, however, was vomiting, which never failed to recur every now and then, and produced much exhaustion. The pain, tension, and vomiting continued with varieties in severity, and not materially affected by the repetition of leeches and blood-letting, till early on the morning of the fifth day, when she sunk.

The uterus was affected with true scirrhus over the whole cervix, and a part of the body. The omentum and intestines, particularly the turns lying in the pelvis near the aperture, were much inflamed. Nothing particular was found in the condition of the remaining organs of generation, except that the left Fallopian tube was distended into a close sac of the size of a hen's egg, and filled with serum. No injury had been inflicted on the bladder, rectum, or any other organ. An attempt was made to discover the source of the hemorrhage. No divided artery could be found, probably because they had retracted; but the open mouths of some divided veins were observed, which were traced to the pelvic plexus of veins, and which, as they possess no valves, *Mr Banner* thinks might have supplied the blood lost immediately after the operation.—[*London Medical Gazette*, ii. 582.]

III. IV. and V. The next are three unsuccessful cases related in very brief terms by *Dr Blundell.* In *Case III.* the usual symptoms of carcinoma of the uterus were well-marked, the disease had affected the vagina, and it was so far advanced, that death appeared likely to ensue within two months more. The particulars of the operation are not given. Six or eight ounces of blood only were lost. The pulse was extremely rapid, the skin clammy, and the debility extreme from the moment of the operation; the patient never rallied, and sunk in thirty-nine hours. On dissection it was found that the disease had been thoroughly extirpated, and that no mischief had been done to any organ in the neighbourhood. In *Case IV.* considerable hemorrhage took place after the division of the broad ligaments; and at the time the uterus was pulled down the pulse became imperceptible. The strength was partially restored for a time by means of opium; but the patient never rallied properly, and died in nine hours. On dissection the organs adjoining the uterus were found uninjured; there was no appearance of hemorrhage to any material extent; but the vessels of the broad ligament, and particularly the veins, were enlarged. Of the fifth Case, we are merely told that it proved fatal a few hours after the operation.—[*London Medical Gazette*, ii. 733 and 780.]

VI. The next case we shall mention has lately made much noise in France, where it happened in July 1829. The operator was *M. Récamier*, physician to the Hotel-Dieu of Paris; and he was completely successful. On this account we shall describe the particulars with some minuteness. The patient was fifty years of age, and had been affected for seven months with slight pains in the lower part of the pelvis, an uneasy lassitude in the loins when she stood, and a fetid, sanious, occasionally sanguinolent discharge from the vagina. The posterior lip of the mouth of the uterus had ulcerated away; the anterior lip was thick, prominent, and ulcerated; and an inch of the upper and back part of the vagina was similarly diseased. The mouth of the uterus admitted the finger into its cavity; the parietes were thickened from the formation of fungous excrescences; but no adhesion could be discovered between the uterus and rectum.

The operation was begun by pulling down the uterus to the external aperture, by means of a pair of forceps *(pinces de museux)* fastened as high on the *cervix* as possible; and the forceps was retained in this position by an assistant. Having ascertained that the rectum had not descended with the uterus, *M. Récamier* passed the left fore-finger, between the fore part of the vagina and the tumour formed by the prolapsed uterus, to the *cul-de-sac* of the vaginal membrane, then slipped along this finger a bistoury, with a round hatchet-shaped point, and having a cutting edge only at its extremity, and made an incision in the *cul-de-sac* an inch in length,—care being taken to keep the cutting edge always towards the uterus, to avoid wounding the bladder. The bistoury was next withdrawn, and the finger thrust through the incision to separate the cellular connexions of the fundus of the bladder with the vagina and neck of the uterus. By moving the finger from right to left, and from above downwards, it arrived at length at the fold of the peritonæum, which passes from the bladder to the uterus. This was divided with the bistoury formerly used, and the incision extended right and left with a blunt-pointed bistoury.

The fore-finger now reached the fore surface of the uterus and broad ligaments, which were felt to be in a stretched state by the dragging of the uterus downwards. The edge of each broad ligament was now divided close to the uterus, till only a third of its thickness remained uncut. The bistoury being then withdrawn, a ligature was passed, by means of a curved needle, round the lower edge of the undivided part of each ligament, then over and round it with the help of the left fore-finger, and was finally secured by means of a *serre-nœud*, or knot-tier. At this stage hardly an ounce of blood was lost, and no farther hemorrhage was apprehended.

The fore-finger was next passed over the uterus, and brought its posterior surface forward and fundus downward through the incision in the vagina. In this situation the bistoury was again passed along the finger, to divide the peritonæum where it passes from the uterus to the rectum, to dissect the cellular connexion between these two organs, to divide the posterior surface of the vagina, and to complete the division of the broad ligaments. After this the uterus had no longer any attachment to the pelvis, and was easily withdrawn. During the separation of the uterus from the rectum, an assistant kept the finger in the gut, and gave notice of the operator's progress.

The portion of vagina removed was two inches and a quarter long behind, and hardly two inches long before; its surfaces of connexion with the bladder and rectum were quite healthy; and its inner membrane was much ulcerated on its posterior surface, though less so anteriorly. The ulceration which had destroyed the posterior lip of the *os tincæ* extended three or four lines beyond the neck upon the body of the uterus; but the uterus was otherwise healthy. At its junction with the vagina there was a large, hard, lobulated tumour, which extended backwards; and this, during the examination by the rectum, had been mistaken for the uterus itself.

At the moment of the removal of the diseased parts, the extremity of the omentum appeared between the nymphæ, and was immediately replaced. The vulva being washed with cold water, a bit of sponge was thrust into the orifice of the vagina, and the patient put to bed with the head and knees elevated, and a large poultice on the abdomen. In the course of the day she had several attacks of vomiting, and at night violent strangury, for which the catheter was used twenty times. A sero-sanguinolent discharge flowed from the parts.

Next day there was at first no change, except some fever, on account of which six ounces of buffy blood were drawn; and in the evening she was considerably easier. On the third day the lower part of the belly was a little distended, and she had slight spasmodic pain, but scarcely any tenderness. In the evening the venesection was repeated. On the fourth the venesection was repeated a third time, and a clyster given with effect. There was more swelling of the belly and occasional nausea, but no tenderness till the evening, when fixed pain was complained of in the right flank. This was removed at once by forty leeches.—On the fifth the ligatures were relaxed. Pain being complained of in the groins, leeches were applied again, and the tepid bath used occasionally.—On the sixth leeches were twice re-applied near the groins. The discharge had become brown and fetid; and the patient for the first time voided urine naturally.—On the seventh there was a marked amendment in her general state.—On the eighth examination with the speculum showed that cicatrization had begun at the end of the vagina. The ligatures were withdrawn.—After this her convalescence went on steadily. The discharge, at first very fetid, was gradually deprived of this quality by frequent injections of cold water, and at length became healthy pus. On the sixteenth day she was able to sit up a little, and in a month more she was able to walk in the garden of the hospital. At this time the upper part of the vagina was drawn together like a purse, and the opening which still remained admitted the point of the finger into a little cavity formed by the fundus of the bladder, and fore part of the rectum. This cavity discharged only a few drops of healthy pus, and nowhere in its vicinity could any hardness or swelling be perceived. Here the account of the case by *M. Récamier* terminates.

Since then some notice has been taken of the woman's state by a French weekly periodical. About six months after the operation she became a patient at the Hotel-Dieu on account of costiveness and some collateral ailments. It was then learned, that she had continued ever after the operation to be annoyed with frequent desire to pass water, to which she was commonly obliged to yield once every hour; that a tendency to constipation which annoyed her before the operation continued after it; and that the inconvenience to which she was thus subjected prevented her from following her former occupation of tambouring. The bottom of the vagina was completely closed at the depth of two inches and a half; and the membrane was moistened by natural mucus only. The indisposition which led her to return to the hospital did not detain her there above a few days. [*Archives Gén. de Méd.* xxi. 78. *Nouvelle Bibliothéque Méd.* 1829, iii. 253, and *La Lancette Française, Janvier.*]

VII. The only other surgeon who in recent times has coveted the reputation of an operator in this line is *M. Roux* of Paris. He has very lately performed the operation twice. The former of his cases occurred in the person of a lady fifty-five years of age, who had been liable to leucorrhœa from her twenty-second till her thirty-eighth year, and who was again attacked in her fiftieth year, soon after the cessation of the menses, with a discharge, which at first appeared to be of the nature of leucorrhœa, but which ere long assumed the characters of the carcinomatous fluid. When examined immediately before the operation, the vagina was found quite healthy and the neck of the uterus swelled, but not hard; a hard fungoid tumour was attached to the mouth of the uterus; and the body of the uterus was felt through the rectum to be irregular and lobulated on its surface, but not enlarged. The patient had no fever, but exhausting pain.

The steps of the operation were for the most part the same as those followed by *M. Récamier*. On arriving at the expansion of the peritonæum from the bladder to the uterus, it was found, as had been anticipated from a previous examination, that the peritonæal covering of the two organs adhered firmly together. In separating them a tumour was discovered about the size of a walnut, which might have been mistaken for the uterus, as this lay behind it, and felt like the extremity of the rectum. The adhesions were destroyed with the fore-finger and the tumour extracted. On next attempting to turn the uterus downwards and forwards as *M. Récamier* did, *M. Roux* found it impossible at first to accomplish his purpose on account of the narrowness of the vagina; and in his efforts a jet of urine was expelled. He consequently determined on dividing the anterior portion of the perinæum, by means of which sufficient room was obtained, and the operation was concluded in half an hour.

For half an hour afterwards the pulse was almost imperceptible and very rapid. After this re-action commenced. In nine hours some pain was complained of in the lower part of the belly, which, however, was not tense. Six leeches were therefore applied, and with relief. But the patient never rallied properly; the pulse remained feeble, her general uneasiness and exhaustion increased, and without any other particular symptom she died thirty-three hours after the operation.

The tumour at the mouth of the uterus was evidently cancerous, the womb itself friable, lardy, soft, somewhat transparent, and four inches long by three at its greatest diameter. Its cavity might have contained a small hen's egg, and was botryoidal. The left ovary presented the appearance of incipient cerebriform degeneration. The right broad ligament and ovary were completely cancerous, and the Fallopian tube distended with cerebriform matter. The bladder presented a circular opening three or four lines in diameter at the summit of the tumour which lay between the uterus and bladder. This opening could not have been made by an incision, otherwise it would have been linear. It must have arisen from the tumour having thinned the parietes of the bladder by its pressure, so that they gave way during the subsequent pressure of the uterus during the attempt to extract it. There was no appearance of inflammation in any part of the cavity of the belly. Only two ounces of a reddish serum were seen in the pelvis.

VIII. *M. Roux's* second case was that of a female thirty-eight years of age who had suffered from uterine pains for four years, and had a reddish discharge from the vagina for six months. The surface of the neck of the uterus was irregular, knotty, very hard, and cracked. The body of the womb was also felt by several physicians who examined her to be considerably enlarged. The same plan of operating was pursued as in the former instance; but, as on that occasion, several difficulties were again encountered. After opening the peritonæum, *M. Roux* experienced some difficulty in reaching the upper edges of the broad ligaments for the purpose of dividing and tying them; and only succeeded after pulling the uterus a little down by a strong pair of double forceps. The enlargement of the uterus rendered the turning of it forwards a very tedious and troublesome part of the operation, which at length was accomplished with a sudden jerk; and at the same moment the operator was covered with blood. The division of the ligaments was then completed, and an attempt made to tighten the ligatures; during which, however, one of them gave way, and no attempt was made to replace it. The whole operation lasted twenty-nine minutes. The patient was pale, and the pulse contracted; but there was not any material hemorrhage. Cold cloths were applied to the vulva and abdomen. The patient never rallied. Her exhaustion went on increasing from hour to hour, though the hemorrhage was insignificant, and she died exactly a day after being operated on.

The neck of the uterus was affected as already mentioned. The body was four inches long by two and a half wide, uniformly hypertrophied, altered in colour, and marked by streaks of disorganization passing up from the diseased part of the cervix. The pelvis contained a little blood. Nowhere was there any appearance of inflammation. The bladder and rectum were entire and uninjured. [*Bulletins des Sciences Médicales*, October 1829.]

IX. *M. Récamier*, looking more to his former success than to the miserable result of his countryman's cases, has still more recently attempted the operation a second time; but on this occasion he has been more unlucky than any other operator. The patient was a lady, thirty-five years of age, who had been affected for some time with uterine pains, and who, on examination by the vagina, was found to have scirrhous uterus with deep ulceration of the *os tincæ*, and some extension of the disease both upwards on the body of the womb and downwards over part of the vagina. It was agreed by several surgeons of eminence, that no chance remained of saving her life except by an operation, to which she therefore gave her consent.

M. Récamier followed the same course as on the former occasion, when he was so eminently successful. But, unfortunately, in dividing the right broad ligament after the uterus was turned with the fundus downwards and forwards, the instrument was applied too near the ligature, which was consequently cut loose. The hemorrhage was arrested for the time by pressure; but it returned in the course of the day, continued to recur from time to time notwithstanding various attempts made to check it by plugging, and proved fatal the day after the operation. In this instance, besides the omentum, several folds of the intestines presented themselves at the gape. A material difference in the present operation too was that the section of the broad ligament was made a little beyond the ovary and Fallopian tube, which were consequently removed with the uterus.

We have now given a faithful analysis of the particulars, so far as they have been made public, of nine cases of this formidable operation. The number is sufficiently great to justify some general conclusions as to the probability of its acquiring a respectable station among the objects of operative surgery.

The first point to be considered is the nature of its immediate dangers. The most direct and obvious of these is hemorrhage. There has been a singular variety in the cases as to the amount and danger of the hemorrhage. Among the five British cases in two only was there any material hemorrhage, and neverthless no precautions were taken during the operation to prevent it. It is plain, however, from the circumstances of these two cases, and of case ninth, the second of *M. Récamier's*, that on the whole mortal hemorrhage, or a degree of it which must be extremely hazardous in the exhausted state to which the patient will always be reduced before any surgeon would dream of operating, is a likely enough occurrence, and ought therefore to be guarded against. The method adopted by *M. Récamier* appears as simple a mode of obviating the danger as can be devised. But a much more serious and immediate danger depends on collapse from the violent nervous impression produced by the operation. It appears somewhat questionable whether this was not fully more the cause of death than the hemorrhage, even where hemorrhage did occur, for the blood lost does not seem to have been in any instance very great; and at all events simple collapse was the cause of death in the greater number of the cases.

Two patients only of the nine recovered. If the surgeon could rely even on that proportion of genuine recoveries, the chance might perhaps be sufficient to justify the general adoption of the operation. But unfortunately there is no satisfactory evidence that either of the cases was an instance of genuine ultimate recovery. So far as may be judged from the small number of cases of this denomination three remote risks must be encountered. In the first place, it seems impossible to have any certainty or rational probability of the disease being confined to parts which can be extirpated. *M. Récamier* and others who are favourable to the operation dwell strongly on the necessity of ascertaining that the uterus is quite moveable; and the French operator in particular cautions those who would imitate him to trust rather to the sensation communicated to the finger in the rectum than to that produced in an examination by the vagina. But may the disease not have extended to adjoining organs, although the uterus remains moveable? And is it always possible to ascertain whether this has happened or not? It may reasonably be suspected that no examination which it is practicable to make will secure the surgeon against this danger. And we confess it is difficult to suppress a smile when we read of the operator pretending to ascertain by fingering the rectum and vagina, that the broad ligaments are free of disease. The reader will perceive the force of these observations by turning to case seventh, one of the unsuccessful attempts of *M. Roux*.—The second remote risk is the probability of the scirrhous disorder appearing in the neighbourhood of the extirpated organ, even when every portion of disease has been removed at the time of the operation. It is not improbable that this occurred in the first case of *Dr Blundell*; although at the same time doubts may be entertained whether some part of the disorder found after death did not exist at the date of the removal of the uterus. The chance of the recurrence of scirrhus cannot be calculated. But it is undoubtedly a circumstance to be taken into account in considering the real value of the operation. Lastly, both the successful case of *Dr Blundell* and that of *M. Récamier* show, that another remote risk is obstruction of the rectum from changes induced in the relative position of the parts in the pelvis by the removal of so material a part of its contents, or by disease brought on in some other manner. This plainly happened in the former of the two; and farther experience will be required before *M. Récamier* can safely pronounce that his patient is exempt from some such event.

No one can be surprised, therefore, that in late years *Dr Blundell* in Britain, and *M. Récamier* in France, have hitherto met with few imitators; and more particularly that, with the exception of M. Roux, no imitator has been found among the many surgeons who have previously established for themselves a reputation for skill and daring in these two countries.

A great difference obviously exists between the danger attending the operation of excision of the uterus from its natural situation, and the removal by ligature of its fundus, or even of the entire organ in cases of reversion or prolapsus. The latter operation, indeed, does not seem to be attended with any material risk, so far as the cases hitherto published will justify an opinion.

It is no part of our object in the present analysis to consider the merits of the several modes of operating which have been tried by different surgeons. We have put the reader, however, in possession of the means of judging for himself. But we cannot help taking notice of an extraordinary proposal which has been made for improving the operation by a foreign contemporary, the editor of the *Bulletins des Sciences Médicales*. This gentleman, who, it is fair to add, seems to hold the operation very cheap, suggests, that, if it is to be practised, a better method would probably be to extract the uterus through the hypogastrium instead of the vagina, as he thinks that the parts will be more easily reached and subjected to much less dragging. We really, for our own parts, do not see how this mode of operating will give the surgeon any superior facilities, or how any advantage is to be derived from making two great openings into the pelvis instead of one.

In the present sketch no notice has been taken of the operation of excision of the neck of the uterus,—an operation which belongs to the same class with those we have already described, and which was proposed some years ago, and continues still to be very frequently practised by *M. Lisfranc* of Paris. The purpose is to remove scirrhus of the uterus in its incipient stage when it is confined to the cervix. The instances in which he has performed it have been extremely numerous, and his success is said to have been most encouraging. It is singular, however, that his success has not hitherto encouraged many of his countrymen, or, so far as we know, any British surgeon to follow his example. We shall probably take an early opportunity of recurring to this subject; meanwhile, we may observe, a general feeling of distrust prevails among operating surgeons in this country of the reality of *M. Lisfranc's* success. It is conceived that the existence of scirrhus is rarely suspected when it affects only so much of the uterus as may be removed by his operation,—that when suspected to exist, its true nature is not always easily determined at so early a stage,—in short, that *M. Lisfranc* must often operate on cases which in Britain would not receive the name of cancerous induration.

ART. IV.—1. *A Catalogue of the Preparations in the Anatomical Museum of Guy's Hospital. Arranged and Edited by desire of the Treasurer of the Hospital, and of the Teachers of the Medical and Surgical School.* By THOMAS HODGKIN, M. D., Licentiate of the Royal College of Physicians of London, Demonstrator of Morbid Anatomy, and Curator of the Museum at Guy's Hospital, &c. &c. 1829. 8vo. Pp. not marked, but contains 31 sheets.

2. *Descriptive Catalogue of the Anatomical Museum of the University of Edinburgh.* 8vo. Edin. 1829. Pp. 245.

NEXT to the collection of objects illustrating the healthy structure and morbid changes of the animal body, a descriptive catalogue is not only of the utmost importance but very nearly indispensable. The mere principle of arrangement, however rude and simple it may be, suggests the necessity of some enumerative list; and if it is difficult to arrange, it is impossible to convert to much use any collection, however valuable and extensive, without such an accompaniment. The facts demonstrated by such collections can thus only be made generally known, and contribute to the advancement and diffusion of scientific information.

The first attempt which was made to communicate knowledge in this manner was that undertaken by Olaus Wormius, the celebrated physician and naturalist, and published at Leyden in 1655 by his son William Wormius, viz. a Catalogue of the Rare and Curious Objects of Natural History, collected by his father.[*] As in the infancy of such undertakings, however, the Wormian Museum was a receptacle not only of objects of natural history, but of Danish antiquities and all manner of rarities, the descriptions of the author are something miscellaneous; and the reader is amused by the sudden transition from the history of the Narwhal and Walrus, to those of the Bird of Paradise, the Penguin, and the Arctic Duck; or from a comparison of the White Bear of the North with the Coati of Brazil, he passes to a discussion on the natural history and anatomy of the Lemming; or after some remarks on the brazen spurs of the ancient Scandinavians, compared with those of Gustavus Adolphus, he is entertained with Runic staves, ancient northern shields, and Hirlas horns. This work is illustrated with very spirited wood-cuts, and a few engravings.

The next attempt of this kind deserving notice had the honour to be made in England by Nehemiah Grew, distinguished as a comparative and vegetable anatomist, who in 1680 published a folio volume on the rarities belonging to the Royal Society, and preserved in Gresham College.[†] To the naturalist and anatomist this is more valuable and interesting than the Wormian Catalogue; and it contains some curious observations relative to the anatomy of the human subject and the lower animals. The preparations relating to the former subject are indeed few, being all comprised in the first section of part first; but they are interesting, and show the attention which was then beginning to be given to the preservation of useful facts. On the structure and history of the lower animals, they are much more numerous; and a large proportion of the volume is devoted to the description of preparations illustrating the osteological structure of the different classes. In the department of shells, several interesting varieties are described and delineated, and of insects some curious specimens are recorded. Like the Wormian collection, however, that of the Royal Society is miscellaneous; and though even then containing many things interesting to the anatomist, it is more important to the naturalist and general inquirer.

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[*] Museum Wormianum, seu Historia Rerum Rariorum, tam Naturalium, tam Domesticarum quam Exoticarum, quae Hafniæ Danorum in ædibus authoris servantur. Adornata ab Olao Worm. M. D. et in Regia Hafniensi Academia olim Professore Publico. Lugd. Bat. 1655.

[†] Museum Regalis Societatis; or a Catalogue and Description of the Natural and Artificial Rarities belonging to the Royal Society, and preserved at Gresham College. Made by Nehemiah Grew, M. D. F. R. S. and of the College of Physicians. London, 1686.

The next attempt of this description was on a much more extended scale; and, accomplished as it was at the close of a life zealously devoted to anatomical pursuits and inquiries into natural history, it demonstrates at once the diligence of the collector, the value of his labours, and the utility of communicating their results to the world. We allude to the work of FREDERIC RUYSCH, who published in twelve successive lists, denominated *Thesauri*, a descriptive catalogue of the preparations of his cabinet, interspersed with numerous explanatory observations. The reasons of this, which are obvious, are stated by the author himself. As the collection of Ruysch was not only the most extensive, but the most valuable and curious of his time, it was frequented by visitors of every rank and description; some in pursuit of knowledge, some in order to gratify curiosity. Its extent, however, was so great, that no one could inspect the whole of the rarer objects contained in it; and his own memory, he tells us, was quite inadequate to remember the peculiarities and situation of each. "By the completion, however, of this catalogue," he continues, "I can forthwith find any preparation which I wish. And if any one, on perusing it, recognizes any object which he is desirous to inspect, he has an immediate opportunity of gratifying his curiosity. Some spectators may be delighted with seeing one preparation, and others are pleased with different ones; and thus I shall endeavour to satisfy all without remarkable loss of time. Lastly, if any one entertains doubts of the existence of the objects delineated by me, he will have an opportunity of obtaining ocular testimony—a facility not afforded by other anatomists, some of whom have obtruded on us numberless fancies, not only remote from truth, but by no means existing in the human body."

Whether the catalogue of the Ruyschian Museum actually answered the purpose last mentioned, it cannot be doubted that it was of infinite advantage to the cause of anatomical science in general. Embracing, as the collection did, anatomy in its three great departments, human, comparative, and morbid, it tended to rectify many errors, and to communicate many new and interesting truths in the former, and in the latter it contributed many important facts, the value of which is undiminished at the present time. It is a point of some interest to the pathologist to remark, that the *Thesauri* of Ruysch contain some of the best representations of diseased bones, and perhaps, excepting a specimen recorded by Job A Meckren about the same time, give the first good delineations of necrosis. These engravings, and their explanations, are indeed durable memorials of the zeal and assiduity of this great anatomist, and form a pleasing relief to the darker shades of a character which has been accused, not altogether unjustly, of gross selfishness and illiberality in concealing knowledge from his contemporaries. To the publisher of the *Thesauri* much indulgence must be given; and if his apparent illiberality in other points precludes gratitude on the score of scientific zeal, the future anatomist may be thankful that the vanity of the author did not allow him to quit the world without bequeathing a memorial so valuable.

The next undertaking of this description was on a scale still more extensive and magnificent. The anatomical school of Leyden, which, as part of a general establishment for education, was, shortly after the liberation of the United Provinces, founded under the auspices of WILLIAM, the first STADTHOLDER, was taught by a succession of anatomists, who, either by general celebrity or their character as teachers, raised it to deserved eminence. Among these are found the names of BONTIUS, PETER PAUU, VAN HORNE, DRELINCOURT and his associate NUCK, BIDLOO, and lastly, the well-known names of RAU, ALBINUS, and SANDIFORT. Though during the incumbency of Van Horne, Nuck, and Bidloo, numerous specimens of natural history were collected, anatomical preparations were occasionally made, and even valuable donations were acquired by the kindness of the celebrated DE BILS, the arrangement and preservation of them does not appear to have attracted particular attention till the time of Rau, who, after spending the latter part of his life in making numerous researches and preparations, by a codicil attached to his will, bequeathed his whole collection to the university.

The acquisition of this munificent and useful bequest, seconded by the representations of Boerhaave, induced the patrons of the university to construct for its reception a suitable cabinet at a great expence; and as the objects required not only arrangement, but enumeration and descriptive notices, his successor, Albinus, was requested to undertake this duty—which, though void neither of labour nor anxiety, he willingly and ably accomplished. This was the origin of the *Index Supellectilis Anatomicæ* of Rau—which forms the first part of the MUSEUM ANATOMICUM of Sandifort.

The collection of Rau formed the nucleus or commencement of one larger and more complete. During the lifetime of Albinus it received numerous valuable accessions, which more than doubled its size. Both were chiefly anatomical; and their various preparations tended to illustrate the healthy structure and morbid changes of the human body.

To the Leyden collection thus enlarged, another addition was made upon the death of WALTER VAN DOEVEREN, the celebrated accoucheur of Groningen, and afterwards professor of physiology and clinical medicine in Leyden. After this event, which took place in the end of 1783, the patrons of the university and the council of the city purchased the collection of preparations made during the course of a long life, assiduously devoted to teaching and practising different branches of the art of medicine.

FREDERICK BERNHARD ALBINUS, who succeeded his father, was in his turn succeeded by EDWARD SANDIFORT, who, on being called to the academical chair, in his lectures paid particular attention to diseases of the bones; and, with the view of illustrating the subject, not only availed himself of every opportunity of improving that department of the collection by personal diligence and the liberality of friends, but he induced the patrons to purchase the collection of LEDEBOER, an eminent physician of the same city.

Of the collection thus aggrandized by successive additions, Sandifort published a complete descriptive catalogue, in two large volumes, in 1793; and it is no exaggeration to say, that a more superb and valuable contribution to the science of anatomy, sound and morbid, had not before been made. * The catalogues of the three separate collections of Rau, Albinus, and Doeveren, are given as the objects are arranged, and relate chiefly to the sound structure of the human body, which they illustrate by numerous valuable preparations. After these, Sandifort arranges the pathological part of the collection in the following order:—1. *Ossa morbosa*, 2. *Partes molles morbosæ*, 3. *Calculi*, 4. *Monstra*, 5. *Varia*; and each preparation as well as each class, he illustrates by a few short clear observations, relating either to the history of the case, or its anatomical and pathological peculiarities, or both. In the department of diseased bones the catalogue is particularly valuable; and if it be less so in the others, it is rather because we have had, since the appearance of the *Museum Anatomicum*, more frequent and extensive opportunities of studying the effects of morbid action on the different textures of the system, than because any attempt has since been made to approach its excellence. It is a performance, indeed, from which the pathological reader derives the greatest instruction, and which forms one of the best commentaries on the organic changes which fall under his observation.

It might have been reasonably imagined that the brilliant example of Sandifort and the Leyden museum would have, at no remote period, called into the field numerous competitors for the same honourable distinction. The museum of the university of Leyden was not the only nor the best anatomical and pathological collection in Europe; nor, it might have been argued, was Professor Sandifort the only individual who was capable of composing such a work. Of the sixty universities of the German states, most have collections of various extent and merit. The four or five of the Italian states have each collections which have been enlarged by the labours of some of the most distinguished anatomists of the eighteenth century. Though Paris has no large collections, it possesses several small ones. Yet among all these institutions none has ever attempted any thing like the undertaking of the Leyden professor. Among the Continental museums, indeed, some small attempts, we believe, have been made to classify and enumerate the preparations; and from the character now given, we make an exception in favour of the museum at Pavia and that at Strasbourg. By the kindness of a professional friend, who visited most of the academical institutions and museums on the Continent some years ago, we have been favoured with a sight of these catalogues, that of the former collection by Scarpa, and the latter by Lobstein. Both consist of short enumerative lists, simply designating the preparations by number and name.

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* Museum Anatomicum Academiæ Lugduno-Batavæ Descriptum Ab Eduardo Sandifort. Vol. i. and ii. Lugduni Bat. 1793.

That of the Pavian collection, which occupies only sixty octavo pages, is prefixed to a short harangue pronounced by Scarpa on the last day of October 1785, on occasion of the dedication of the Anatomical Theatre, in which the professor gives a short sketch of the progress of anatomy, and the rise of anatomical collections. The catalogue is divided into five sections, Osteology, Splanchnology, Neurology, the Organs of the Senses, and Angiology, followed by a few models, and some microscopical preparations. *

The Strasbourg Catalogue, which is comprehended in about seventy octavo pages, is published as an appendix to a Report given to the Medical Faculty of that city on the actual state of the Anatomical Museum. Its objects are arranged in two great divisions, Physiological or Healthy Anatomy, and Pathological or Morbid Anatomy. The former is subdivided into the following parts;—the osseous system in man and animals; the organs of digestion in man and animals; the muscular system; the system of circulation; the organs of respiration and voice; the urinary organs; the brain, nerves and organs of sensation; and lastly, the sexual organs. The second, or pathological department, is divided into diseases and injuries of the osseous system; diseases of the muscles, the blood-vessels, and lymphatics; diseases of the heart and great ves-

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* Index Rerum Musei Anatomici Ticinensis. Accedit Antonii Scarpæ in Solemni Theatri Anatomici Ticinensis Dedicatione, Oratio, Habita pridie Kalend. Novembr. An. 1785. Ticini, 1804.

sels; those of the pleura, lung, and thyroid gland; those of the organs of digestion; those of the urinary passages; those of the nervous system; those of the organs of generation and monstrous products of that function; and lastly, new products and forms of organization. *

In the arrangement of these catalogues there is nothing to commend,—a circumstance which we mention, not as a censure on the authors, but rather as indicating that the circumstances of their respective collections probably precluded very neat or scientific arrangement. The collection of the Pavia establishment is chiefly relative to healthy anatomy, in the minute and rare departments of which it contains many valuable preparations. The pathological department of the Strasbourg collection, and especially that of diseased bones, is highly valuable and interesting. Next to these are the diseases of the digestive organs; and the collection of monstrosities and congenital malformations is good. The sketch which Lobstein premises to this catalogue is further instructive, and exhibits a useful general view of the present state of pathological anatomy.

In this country, museums, as on the Continent, have generally at least originated in the zeal of individual teachers: and though they have afterwards been fostered by public or corporation patronage, little inducement has been held out to the communication of their stores for purposes of general utility. Of the rich and extensive cabinet of John Hunter, now the property of the College of Surgeons, and perhaps at once the most useful, as the most splendid collection in the world, no catalogue has yet been given; nor has the noble bequest of his brother to the University of Glasgow shared a better fate. However open to the profession these valuable collections may be, they are infinitely less useful than they might be, were good descriptive catalogues of their stores published. The museum of the College of Surgeons of this place, which is now extensive and valuable, is in a similar predicament; and though it has been recently thrown open to the inspection of the profession, this measure, liberal as it is, must be deficient of its full utility, without the concomitant advantage of an explanatory catalogue. We must not forget to mention, that a list of the osseous preparations of this cabinet has been lately given by Mr Benjamin Bell, as a good commentary on his treatise on the diseases of that system.

We have now, however, to direct the attention of our readers

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* Compte Rendu à la Faculte de Medecine de Strasbourg, sur l'état actuel de son Muséum Anatomique suivi des Objets qu'il Renferme. Par Jean Fréderic Lobstein, Professeur d'Anatomie Pathologique, et Directeur du Muséum Anatomique. Strasbourg, 1820.

to more agreeable prospects in this useful branch of professional education. Within the last twelve months there have appeared catalogues of the preparations contained in two establishments of this description,—one from the museum of Guy's Hospital in London, the other from that of the University of this place. Of these two works, though the objects are nearly the same, and the general plan upon the whole not very dissimilar, yet there are certain points of difference which claim notice.

The establishment at Guy's Hospital, though the most recent in origin, has acquired, if we may judge from the catalogue, the more valuable and extensive collection of the two; and it may not be unseasonable to advert to the circumstances of its formation and progress within the space of less than thirty years to its present amplitude.

Though in Guy's, as in every extensive hospital of a populous city, opportunities for anatomico-pathological inquiries are numerous and abundant, it does not appear that they were turned to any permanent advantage previous to 1802; and accidental circumstances rather than design appear to have then suggested the foundation of an anatomical cabinet. At this period, when apartments were appropriated for demonstration and the inspection of morbid bodies, Mr N. Davie, who is commemorated as a zealous and intelligent young man, was demonstrator; and during his lifetime he had collected several preparations of sound and morbid anatomy, which after his death were purchased by the treasurer as the commencement of a museum. These, with a few specimens occasionally obtained in the hospital, constituted a small collection, which was employed to illustrate the medical lectures delivered in the theatre of the Hospital.

In 1806, when Mr Travers was demonstrator, among sundry regulations made by the treasurer to promote the interests of this part of the institution, it was directed that all specimens of morbid structure met with in subjects either dissected by pupils, or inspected at the request of the medical officers, should be preserved as the property of the hospital. The accessions were, notwithstanding, byno means considerable either in number or importance, in consequence probably of the circumstance, that no one was particularly invested with the duty of preservation. In 1824, this defect was supplied by the appointment of a curator or conservator, to whom was also understood to be consigned the care of making casts of cases, in which peculiarities of mechanical figure rendered this requisite.

With all these arrangements, aided on every occasion by the superintending influence of the treasurer, Benjamin Harrison, Esq. the museum appears to have advanced rather slowly. In point of fact, it does not appear to have contained at this period more than 500 preparations; and of these the greater part related to morbid anatomy, the departments of physiological and comparative anatomy being scarcely more than in existence. About this time, however, the schools of St Thomas and Guy's were separated; each were extended and enlarged; and the museum of the latter having thereby become an object infinitely more important, the efforts of all connected with it, but especially of its treasurer, Mr Harrison, were directed with increased energy to its extension and enrichment. To the latter gentleman, indeed, who is the most perfect model of a treasurer to a public institution, belongs the merit not only of founding the institution, but by his unremitting attention of carrying it onward through every difficulty and impediment. It is irrelevant to the present subject to enter into the extensive improvements which this gentleman made in the establishment in general, and the large pecuniary advances which their introduction required. But it would be improper to speak of the Museum without saying, that in every thing connected with its formation and progressive extension, he has made the most extraordinary exertions, and evinced the utmost willingness to render it beneficial to the pupils. For constructing the necessary apartments and providing suitable accommodation, he made the most liberal advances from his own resources. Ready to listen to every rational suggestion for facilitating the means of enlarging and enriching the museum, he adopted every arrangement, and entered warmly into every measure which seemed calculated to attain that object. In short, while to Mr Harrison belongs the originality of the idea of improving the school of the hospital, by attaching to it an anatomico-pathological collection, and of fostering the growth of the Institution with truly paternal care, he has, by accommodating the didactic department to the present state of knowledge, and the claims of medical education, taken the most direct means to augment the utility and elevate the character of the establishment over which he presides.

With these views, the organization of the museum upon its present footing appears to have been commenced in 1826, by the appointment of Dr Hodgkin to the office of demonstrator and conservator of the museum; and it is chiefly to the unremitting attention of this gentleman, next to the liberal support and co-operation of the treasurer, that the collection is indebted for its present extent. From this period, indeed, the museum advanced with rapid steps; and from the comparatively infant and imperfect state above-mentioned, it has, in the three departments of sound, morbid, and comparative anatomy, attained a degree of amplitude and perfection, to which we may safely affirm, there is in the history of scientific collections no parallel. That this is no exaggeration, we believe will be admitted, when we state, that, from the small number of 500 preparations, its condition in 1826, it had attained, in the space of about four years, the extent of 3200, the number in 1829 at the date of the publication of the catalogue; and since this period, we have learnt from good authority, that they have risen to 4000.

All this, no doubt, could not have been accomplished by any single individual. Museums, like other establishments, require not only large pecuniary resources, but the co-operation of several, and are often powerfully aided by the scientific zeal and kindness of contributors. Among those to whom the museum of Guy's appears to have been most indebted in the latter mode, Sir Astley Cooper is especially distinguished as having set the example of preparing numerous valuable specimens with his own hands. With the same laudable intention many other professional gentlemen lent their aid; and those who could not assist in actual preparations, supplied, nevertheless, morbid parts as they occurred in practice. Originally, Dr Hodgkin appears to have not only made and reported all the inspections, but to have completed the parts for preservation. As the collection became extensive, however, he was assisted in this department by the appointment of a modeller, two assistants for dissecting, and a transcriber for recording the reports,—arrangements for which he was also indebted to the kindness of the treasurer, and which enabled him to bestow more efficient attention on other parts of the enlarging establishment. To render it as complete as possible, the treasurer attached to the museum an able artist as modeller, who has been so successful in this department, that his works are supposed by good judges to rival those of the most eminent masters of Florence and Bologna; and lastly, with the view of supplying the defect sustained by loss of colour, which all preparations undergo, Mr Canton is constantly employed in taking coloured delineations of the recent specimens of disease. It is easy to see that with such ample means and so much cooperation, the efficiency of the museum, as a valuable source of professional instruction, could not be doubtful; and it must be admitted, that though of recent origin, and perhaps neither so rich nor so extensive as some older establishments, it is already perfectly equal to all useful purposes, and must at no remote period become not merely the first institution of the kind in this country, but superior, perhaps, to any of those on the Continent.

To realize the utility of an establishment so extensive, and so well provided with means of aggrandizement, a catalogue of its stores was indispensable; and among other duties, that of digesting and publishing a catalogue of the whole collection, though daily becoming more difficult the longer it was deferred, was nevertheless rendered necessary, to enable the pupils to derive from it the advantages which its original institution was intended to afford. This, of course, was assigned to the conservator, who, by long familiarity with the subjects, was well qualified to do it ample justice. He appears, indeed, to have formed a more extensive plan than that which he has actually accomplished; and it was his intention, by introducing descriptions of morbid changes not exemplified by the specimens of the collection, to render the catalogue a general practical repository of anatomico-pathological knowledge. This design, however, which perhaps was too extensive, he has for the present relinquished, and satisfied himself with a mere catalogue of the actual preparations contained in the collection.

The objects enumerated in the catalogue, like those of the collection, are arranged under three general heads, Special or Descriptive Anatomy, Morbid Anatomy, and Comparative or Animal Anatomy. The two former heads are arranged in sections on the same plan, and as much in the same order as the circumstances and the number and kind of preparations admit. Thus, the first of both is devoted respectively to the illustration of the anatomy of the skeleton and its parts in the sound and morbid states. The second section includes the preparations of all the soft parts connected with the skeleton in the sound and morbid states, for instance, cartilages, ligaments, synovial membranes, fibro-cartilages, and the muscles with their tendons, fasciæ and bursæ. To the third section are referred the preparations illustrating the anatomy and diseases of the heart, and the three branches of the vascular system, arteries, veins, and lymphatics. The fourth section contains the preparations illustrating the structure and morbid changes of the nervous system, and the organs of sensation, including under the former head the spinal chord, the brain and cerebellum, the nerves of the cerebro-spinal and gangliar orders; and under the latter the skin as the organ of common sensation and touch, and the organs of proper sensation, or sight, smell, hearing, and taste. In the fifth section are placed the preparations belonging to the organs of voice and respiration in the sound and morbid states; and the sixth contains those of digestion with their accessory organs, the liver, spleen, and pancreas. The seventh section comprehends the preparations relative to the structure and diseases of the urinary organs, with all the varieties of urinary calculi, arranged according to the principles established by Dr Prout. The organs of generation form two sections, the eighth and ninth, the former devoted to those of the female, the latter to those of the male, with their respective diseases.

In the subsequent arrangement of the sections of the physiological and pathological divisions, some difference is observed. In the department of sound or physiological anatomy, the tenth section contains a few preparations illustrative of utero-gestation; the eleventh is devoted to those illustrating the qualities of the fluids, partly in the sound and partly in morbid conditions; and the twelfth contains a miscellaneous assemblage of skeletons, skulls, separated bones for demonstration, skeletons of several of the lower animals, a few injected preparations, and a number of models and casts.

In the pathological department again, we find the tenth section devoted to the morbid changes incident to the peritonæum, and the preparations illustrating the principal varieties of hernia. The eleventh section of the pathological corresponds to the tenth of the physiological department, in exhibiting the preparations illustrating the morbid deviations of the reproductive organs and their products, in the human subject and lower animals. The twelfth section is entirely pathological, being devoted to parasitical animals; and the thirteenth, which is the last, contains an extensive collection of casts and wax models, which are supplementary to the foregoing sections, and are therefore subdivided in the same order.

The department of the collection allotted to comparative anatomy, which constitutes the third part of the catalogue, is subdivided and arranged according to the classes and orders of the animal kingdom, as improved by Cuvier. This is acknowledged to be the least extensive department of the museum, and it is published chiefly as a guide to the student who is desirous to avail himself of the preparations under this head, so far as they go.

In the two first departments of the catalogue, most of the sections are introduced by general observations on the intimate structure and anatomical constitution of the parts illustrated. In those on the healthy structure, Dr Hodgkin makes considerable use of the microscopical observations which, in company with Mr Lister, he made some time ago on the atomic constitution of the tissues and fluids of the animal body. In those on the morbid changes, he restricts himself in general to the mention of the most remarkable cases and examples of morbid action and structure which the preparations afford. While we feel it impossible easily in the limits assigned to this article to enter minutely into the merits of these observations, we must say that they manifestly proceed from one who has studied the subjects of healthy and morbid anatomy with assiduity and success, and who has formed very just ideas of the distinctive characters of both—an accomplishment of the utmost importance to the conservator of an anatomical collection.

The plan on which the individual preparations are enumerated and distinguished deserves particular notice, since it combines the advantages of descriptive arrangement, with authentic references to the history of the case, and the name of the individual by whom it was presented or prepared. Each page is divided into four columns, in the first of which is placed the number of the preparation as it stands in the museum; in the second its anatomical or pathological designation, with a short notice of its physical peculiarities; in the third the reference to the history either in the inspection records, or in notes accompanying the case; and in the fourth and last, the name of the individual by whom it was presented. The latter circumstance, we take this opportunity to say, is too much neglected in arranging the preparations and specimens of collections. It is an idle objection to say that it gratifies vanity. If the feeling of being distinguished as the donor of a valuable specimen or interesting preparation partake of vanity, it is at least a very harmless form of the feeling, and one which is allied to some of the more noble qualities, the operation of which is highly useful to society. The truth is, that so long as the human mind consists of its present elements, so long will gratification be derived from the consciousness of being eminent for zeal in science, and diligence in the pursuit of the means by which its progress is promoted; and it is both idle and injudicious to resist the operation of a feeling so natural. On this account, we think the plan adopted in the Catalogue of Guy's Hospital cannot be too much commended.

A word on the general character of the collection and its catalogue must conclude this notice. It is chiefly in the pathological department that the great strength of the collection appears; and we believe that it is chiefly in this that its utility as a means of instruction will be evinced. The number of the preparations in this department exceeded 1500 at the date at which the catalogue was finished, and they may at the present moment be estimated at 2000. Of these it is difficult to say which set is most valuable. The specimens of morbid and injured bones are, as in most collections, numerous, and, on the whole, rather varied. In point of extent, the specimens of disease of the digestive organs come next. And perhaps the most extensive of all is the collection of examples of disease in the urinary and genital organs.

The second publication of this nature is on a much smaller and more limited scale. The collection, indeed, of which it forms the inventory, is by no means so extensive as that of the great institution in the metropolis. It cannot, indeed, receive any other denomination than that of a mere list or inventory of the preparations. As such, however, it is not only better than no catalogue, but it answers the purpose for which it was intended; and as on this ground it merits thanks, these shall not be withheld.

The history of the Museum of the University of Edinburgh is short and easily told. From a prefatory notice by the present professor of anatomy, we learn that, previous to 1800, there was no museum the property of the establishment; and the preparations used for lecture and demonstration were the private property of the first and second Drs Monro. This collection the latter of these gentlemen presented to the university, "to be used by himself and his eldest son, then his colleague, during their lives, and afterwards by their future successors in office, for the purpose of demonstrating and explaining to the students of the university, the structure, physiology, and diseases of the human body."

The museum thus attached as inalienable property to the university, naturally fell under the care and superintendence of its patrons, who forthwith adopted measures for opening it to students and the profession, and converting it into an efficient source of professional instruction. At the suggestion of Mr Wood the president, and Mr David Hay, one of the fellows of the College of Surgeons, they granted L.100 annually for its support and extension, including the salary of the conservator; they sanctioned the payment of certain fees for admission by the students; and, finally, with the view of rendering the museum useful to visitors, they directed the preparation of a catalogue to be printed at their expence.

Of late years, the collection has been considerably enlarged by preparations with explanatory histories from several of the medical professors and others. From these histories, and from those attached to the preparations of the original collection of the two first Drs Monro, the present catalogue has been compiled by Mr Mackenzie the conservator.

The plan of the catalogue is sufficiently simple, and in general arrangement it bears some resemblance to that of the museum at Guy's, though without separating the physiological and pathological departments. The preparations are arranged and enumerated according as they illustrate the textures and organs of the animal body, first, in their sound, and then in their morbid states, in the following order;—the bones; ligaments, tendons, and muscles; teeth; lips, tongue, and salivary glands; alimentary canal, with intestinal worms and concretions; assistant chylopoietic viscera; absorbents; heart and blood-vessels; organs of respiration; the brain, followed by the organs of sense, the nose, eye, ear, and skin; tumours,—encysted, steatomatous, sarcomatous, medullo-sarcomatous, carcinomatous, subcutaneous; urinary organs; urino-genital organs; the female generative organs; and, lastly, monstrous productions.

Into the merits of this arrangement and its details we do not propose to enter any more than into those of the former catalogue. The arrangement is not free from objection, and it would be easy to point out mistakes; but we are aware of the difficulty of classification, which is always augmented, where the objects to be classified are not so numerous as to complete the system. The best commendation that can be bestowed on the present performance is, that it is sufficienly clear and explicit to act as a guide to those who frequent the museum; and if every similar institution had such a catalogue, they would be infinitely more useful than they at present are. Some of the examples of organic change are well, and rather minutely, described for such a list; and the pathologist will gratify his curiosity by comparing them with the preparations which they describe. These remarks apply especially to the specimens illustrating the diseases of the arterial system, of the respiratory organs, and of the alimentary canal.

In conclusion, we take the present opportunity of observing, that we trust that the example now set will be not merely imitated, but if possible exceeded. The catalogues now published, valuable as they are, are to be viewed in the light of tentative undertakings,—imperfect and infantine as the institutions from which they have issued. To these, however, belongs the merit of showing an example, which should be followed by the other museums of this country. How much the knowledge of the nature and effects of morbid action might be advanced and diffused by similar catalogues from the collections of the Colleges of Surgeons of London, Dublin, and Edinburgh, and from that of the Hunterian Museum of Glasgow, it is needless to attempt to show. The undertaking is both difficult and expensive; but while it requires the patronage, it is surely not unworthy of the dignity, of public bodies.

ART. V.—*Hospital Facts and Observations, illustrative of the Efficacy of the new remedies Strychnia, Brucia, Acetate of Morphia, Veratria, Iodine, &c. in several morbid conditions of the system; with a comparative view of the treatment of Chorea, and some cases of Diabetes, a Report on the Efficacy of Sulphureous Fumigations in Diseases of the Skin, Chronic Rheumatism, &c.* By JAMES LOMAX BARDSLEY, M. D., Physician to the Manchester Infirmary, &c. Burgess and Hill. London, 1830. Pp. 223. 8vo.

THIS is a treatise of peculiar merit, and of a kind of which it is surprising that we possess so few specimens. Within the last fifteen years the materia medica has been enriched, chiefly through the activity of pharmaceutic chemists, by the addition of many new substances, for the most part belonging to the vegetable kingdom, and endowed with a very energetic action on the animal system. Two of these, quina and iodine, are now completely established as acknowledged articles of the materia medica of every civilized country; and ample evidence has been supplied of the efficacy and other advantages of at least the former of these. The remaining substances to which we at present allude, are for the most part vegetable principles derived from certain poisonous drugs, which have long been used more or less extensively in the treatment of diseases. And as these principles concentrate in themselves all the leading physiological properties of the crude drugs, it has been reasonably imagined that they will also possess in an eminent degree their leading therapeutic virtues.

The activity of the chemist, however, has not hitherto been met with a corresponding zeal on the part of the physician; and the profession is still in a state of uncertainty as to the therapeutic merits of the greater number of the energetic principles which have been lately discovered. The experiments which have been made with them down to the present time are not numerous, and far from satisfactory; and, what is worse, we fear they are not very encouraging. It is of very great importance, therefore, that farther trials be made, in order to determine as speedily as possible how much the materia medica is to gain by the late researches in pharmaceutic chemistry. We need hardly add that hospitals form the only satisfactory field of observation, as it is there only that prudent experiments can with propriety be conducted on a scale sufficiently extensive.

Dr Bardsley seems to be fully aware of this, and has made ample and judicious use of the opportunities which his situation of physician to various institutions in Manchester has afforded him. His trials have been directed principally to the most energetic substances, and in some departments his researches have been far more extensive and conclusive than those of any previous observer. His experiments have been extended to *Strychnia*, the alkaloid of nux vomica, *Brucia*, the alkaloid of the False Angustura bark, *Morphia*, the alkaloid of opium, *Quina* and *Cinhonia*, the alkaloids of Cinchona bark, *Iodine, Gentianin* and *Emetin*, the principles of Gentian and Ipecacuan, and *Veratria*, the alkaloid of the meadow-saffron and white hellebore. To the results of his trials with these articles he adds some interesting information as to his experience in the treatment of chorea and diabetes, and the employment of sulphureous fumigations in various disorders.

Among these researches he assigns with justice the first rank to his experimental observations on *Strychnia*.

In 1816 *M. Fouquier* of the Hospital of La Charité at Paris gave a full account of a series of cases illustrative of the effects of extract of nux vomica in the treatment of palsy in its chronic stage.* Since then several physicians have resorted to the practice recommended by *M. Fouquier*, but with variable success. Some have followed his example literally, and have used the extract of nux vomica; others, for the sake of greater certainty in the dose of so active and dangerous a drug, have had recourse to its alkaloid, the strychnia; and in both ways cures have been repeatedly accomplished. Nowhere, however, is there to be found so extensive and thorough an inquiry into the merits of this remedy as in the work now published by *Dr Bardsley*. And it is with much satisfaction we announce that the result is in the highest degree favourable, and that no other method of treatment appears to promise so many chances of success in this intractable disease.

The cases of palsy reported by *Dr Bardsley* are thirty-five in number. We shall consider the results he has obtained with hemiplegics and paraplegics separately, the former of whom constitute twenty-two and the latter thirteen of the total number treated.—Of the cases of hemiplegia seven were of not more than two months' standing; and of these five were cured and two relieved. The age of these patients was from thirty-six to sixty-one. The duration of the treatment was six weeks in two instances, two months in two, and three months in three. Four cases were of three months' standing, and all recovered, except one who left the hospital before the treatment was carried to a sufficient extent. The ages of these four varied from twenty-nine to fifty-four. Six others had been from four to six months ill, and their ages varied from forty-four to sixty-two. Of these three were cured, and three relieved, one very materially. Another who had been seven months ill was relieved. Of two who had been twelve months ill one was cured after three months of treament; and of two who had been eighteen months ill both were cured by two months of treatment. Of the total number twelve were cured, eight were considerably relieved, one received no benefit, and one left the hospital while taking the strychnia.—Of thirteen paraplegic cases, a few of which were about four weeks' standing, but chiefly of twice that duration, no fewer than eleven were cured, one was slightly relieved, and one received no benefit. The case which received only partial relief had existed two years. One of the cures occurred in the instance of a lad who had been paraplegic for eight months, another in that of a man fifty-nine years old, who had been similarly affected for a twelvemonth. The treatment was rarely prolonged beyond two months. It is a remarkable circumstance that three of the cures occurred in the instance of persons rendered paraplegic by falls.

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* Bulletins de la Fac. de Méd. de Paris, 1816, No. 9.

These numerical statements appear to furnish strong evidence of the curative effects of strychnia in some forms of palsy, as more than a half of the hemiplegic, and six-sevenths of the paraplegic cases recovered. But the physician cannot too much accustom himself to look for other evidence of the remedial powers of a drug than mere apparent success. In the present instance such evidence is not wanting. On the one hand, we have evidence that the cures were owing to the strychnia, and not to nature or concurring causes, in the circumstance that paraplegia has been removed with far greater certainty than hemiplegia. This is exactly what might have been anticipated from the known physiological action of this substance, which acts much more energetically on the spinal chord than on the brain. On the other hand, we have still more convincing evidence of the reality of its therapeutic effects in the circumstance, that the commencement of the cure invariably corresponded with the first manifestation of its physiological action. This is perhaps the strongest species of proof which it is possible to procure in favour of the reality of the operation of any remedial agent; and in the instance of strychnia as a remedy for palsy it presents itself in the most unequivocal form. In every case in which it was successful in Dr Bardsley's hands—and we have remarked the same thing of other cases which have been published by previous observers,—the first appearance of amendment was immediately preceded by twitches of the muscles, sometimes of the palsied limbs, sometimes of the healthy limbs also; and if the dose of strychnia was increased after the appearance of this symptom, the twitches assumed more and more the character of the spasmodic convulsions produced by poisonous doses.

We conceive, therefore, that there is no reason for calling in question the therapeutic power of strychnia in this disease. It was rendered probable by the observations of *Fouquier* and others; and the experience of *Dr Bardsley* goes far to establish it.

But an important question for consideration is its safety. Is it safe to administer so active a poison at all? and is it safe to administer it with the view of stimulating the brain and spinal chord in a disease, the cause of which is very frequently an organic change of structure, the consequence of inflammation?

As to the former of these questions, the experience of several physicians who have made extensive use of the extract of nux vomica or its alkaloid, clearly shows that, although there can never be any safeguard against the risk of poisoning through ignorance or accident, yet the doses may be so regulated as in careful hands to avoid the risk of its poisonous effects being developed in an alarming degree. In several of *Dr Bardsley's* cases, when it was carried too far, the patients were affected with sinking of the pulse, uneasiness in the præcordia, obstructed breathing, giddiness, vomiting, contraction of the palsied parts; and in one instance "violent spasmodic action of the muscles of the chest and lower extremities ensued, during the continuance of which he was almost unable to speak or move, and breathed with extreme difficulty." Such accidents show the necessity of great caution in increasing the doses. For, on the one hand no benefit can be expected unless the physiological action of the drug is obviously developed; and, on the other, any increase of that action beyond the simplest manifestation of its existence is attended with serious and possibly dangerous symptoms. Those who resort to the strychnia, therefore, must take care, as *Dr Bardsley* cautions them, to watch the effects of every dose narrowly when the quantity has been increased nearly to the extent at which its action may confidently be looked for. The author generally began with a sixth of a grain twice or thrice a-day; and he found that on gradually increasing it he seldom reached half a grain thrice or a grain twice a-day without spasmodic symptoms manifesting themselves. An interesting fact which deserves mention here, and which appears to us to result from his observations is, that the system does not become habituated to the use of strychnia as to opium, so as to require a gradual increase of the dose to produce the same effects, when it is long continued.

But, secondly, is it safe to administer strychnia in paralytic affections? For it is well known to act as a powerful stimulant on the brain and spinal chord, and as such indeed was first employed in the treatment of palsy by *Fouquier*; while this disease very often originates in extravasation or inflammation of the brain or spine, which terminate in organic changes, and are very apt to be renewed by powerful excitement. On account of this risk the use of strychnia and nux vomica has been proscribed in all cases of recent palsy, and likewise wherever the paralytic affection is accompanied by frequency of pulse, headach, giddiness, or other signs of increased action within the head. *Dr Bardsley*, though evidently quite aware of these circumstances, does not seem to have attached much consequence to the possibility of encountering organic derangement of the brain. When any local symptoms indicated the continuance of determination to the head or other active process, he used the precaution of resorting in the first instance to local blood-letting and other appropriate remedies; and when these were subdued, he had recourse fearlessly to the strychnia, even in cases of short standing. He does not appear to have met in any instance with indications of undue cerebral excitement or aggravation of the disease. It is almost unnecessary to add, however, that this remedy was never employed in any acute case of palsy accompanied with inflammatory action.

Dr Bardsley has likewise used strychnia in two other diseases; but in them it is by no means so easy to satisfy one's self of its beneficial operation. These diseases are *chronic diarrhœa*, and *amenorrhœa*. In some instances of chronic diarrhœa, where the stools were feculent, but thin and frequent, unaccompanied by pain, to all appearance therefore unconnected with inflammatory action, and tending towards a fatal termination through the mere exhaustion produced by the constant drain, he found small doses of strychnia slowly restore the healthy action of the bowels, after all other remedies in common use, of a sedative and astringent nature, had been tried in vain. In several cases too of obstinate amenorrhœa he employed it with apparently signal success. He has given the particulars of several cases and the general facts of twelve, from which it appears that ten were cured, and the two others relieved, that in some instances the catamenia had been retained for eighteen months, and that the usual duration of the treatment seldom exceeded two months and sometimes did not surpass three weeks.

The next subject of inquiry is the therapeutic virtues of *Brucia* in palsy. This alkaloid, which is procured from the *Brucea antidysenterica*, or False-angustura-bark, possesses precisely the same physiological properties as the strychnia and its natural compounds, but is not nearly so active. It was reasonable therefore to expect that it would be of service in the treatment of palsy, and accordingly *Dr Bardsley* seems to have found it not less serviceable than the strychnia. He has related the heads of ten cases which were treated with the Brucia, among which eight were cured and two relieved. But he states that these have been selected from a larger number as best illustrating the therapeutic properties of the drug; so that he has not supplied the means of judging of the chances of cure. This, we must take the liberty of remarking, is a material omission.

The age of most of the patients was between forty and fifty, the duration of the disease between four and six months. Seven were cases of hemiplegia, and three of paraplegia: and of the former, five were cured and two relieved, while all the paraplegic patients were cured. One of the paraplegic cases was produced by a fall. In most of the cases the treatment was persevered in for nearly three months; but *Dr Bardsley* very properly remarks, that if a sensible amelioration does not take place after five or six weeks of treatment, the alkaloid being given to the requisite extent, there is no chance of its doing any good. The dose he usually began with was one grain twice a-day, and it was gradually increased every sixth or tenth day, till two grains were taken thrice or four times daily. But this amount was seldom attained, and never surpassed, without some signs of its physiological action appearing. In one instance, the administration of two grains five times a-day caused considerable cerebral disturbance. At one time it excited giddiness, pain in the stomach, griping and palpitation, and again indistinctness of speech, and some headach, for which the author judged it prudent to abstract blood by cupping, and to diminish the dose.

Dr Bardsley next passes to a topic of very great importance, the effects of the *Acetate of Morphia.*

In the number of this Journal for October 1826, we attempted to ascertain the amount of the experience of the profession as to the usefulness of Morphia in a review of some clinical observations by *Dr Quadri*, an Italian physician. Since then, we have met with nothing in print worthy of particular notice, except a detailed account by *M. Bally* of the results of his experience, to which we alluded in the notice of Dr Quadri's papers. On the whole, with the exception of Dr Quadri's paper, the general scope of the researches of these and some other authors of less note is, that the usefulness of morphia as a substitute for opium is doubtful. It appears probable, notwithstanding the early statements of *Magendie* to the contrary, that morphia sometimes induces the same unpleasant idiosyncratic and secondary effects as opium itself. It would be very desirable, however, that this question were settled once for all; because no one can doubt that a preparation free from these troublesome effects, and retaining the hypnotic virtues of opium, would be one of the most valuable gifts which pharmaceutic chemistry could present to the physician.

Dr Bardsley has made trial of its effects in affections of the stomach, in uterine diseases, and in neuralgic complaints, and his opinion is very favourable.

He first relates the particulars of ten cases of dyspepsia, in which the acetate of morphia was given with complete success. In some of these the symptoms were of the usual kind, indigestion, eructation, costiveness, pain in the stomach, emaciation, languor, and debility: Some were cases of neuralgia of the stomach, attended with loss of appetite, and one was a case of pyrosis. The acetate of morphia was given in the form of pill, in the dose of a quarter of a grain at first, and afterwards of a half, and even a whole grain, repeated generally three or four times a-day, but occasionally so often as once every other hour. In every instance it produced very great abatement of pain, and a gradual improvement of the digestive powers, with return of appetite and strength; and a very remarkable circumstance in its action was, that instead of costiveness being induced by it, the bowels continued perfectly regular in every instance but one during the whole period of its administration, and it was only necessary to administer laxatives at the commencement, to remove the fæces which had been accumulated by previous neglect. This particular effect is certainly one which would not have been anticipated from a substance which possesses over the muscular system the same paralyzing influence which is possessed by opium. In the instances in which we have ourselves employed the acetate of morphia, we have not observed this immunity from constipation. It is singular that *Dr Bardsley* is silent as to the narcotic or hypnotic effects of the drug in stomach complaints. Yet he sometimes administered it in doses which must have developed these effects. In one instance he gave a grain every third hour. In the cases of affection of the stomach which got well in his hands, it was seldom necessary to prolong the treatment beyond a month or six weeks; and the commencement of amelioration took place at a very early period. In some instances the patients had previously used opium, but had been obliged to give it up on account of its constipating effects.

After these cases are related several examples of the beneficial operation of acetate of morphia in painful diseases of uterus. Most of the patients of this class laboured under scirrhous uterus, and the remedy was given merely to assuage pain, which it appeared invariably to accomplish, and in general without constipating the bowels. One case, an instance of fetid uterine discharge, accompanied with severe hypogastric pains, got quite well under its use. Another, a case of painful menstruation, derived complete relief from periodical returns of great suffering, by taking a quarter of a grain every other hour at each menstrual period, as soon as the attack of pain commenced.

Next follow a set of selected cases of *Neuralgia* of the face, in all of which the same remedy was of material service. Six cases are related, two of neuralgia of the infra-orbitary nerve, two of the inferior dental, one of the supra-orbitary, and one of an entire side of the face. Several were of some months standing, and all very severe. Most had been subjected to a variety of methods of treatment, including the carbonate of iron, and even division of the nerve, without any amendment resulting. In all the acetate of morphia, in the dose of a quarter of a grain, or from that to one grain, taken every half hour, or every other hour, according to circumstances, gave obvious and immediate relief; and all were ultimately cured, some in seven or eight days only. In this disease, as in the two former affections, it was remarked that the bowels were not rendered torpid.

Dr Bardsley concludes this subject of investigation, by stating, that he never witnessed any pernicious effects from the employment of acetate of morphia; that it seems to him peculiarly adapted to such cases as require the production of narcotism without the supervention of costiveness; and that its use has in his hands been unattended by distressing headach, or any of the other unpleasant symptoms usually caused by opium. In short, his trials are decidedly favourable to the introduction of this substance into the materia medica. We therefore hope to see it soon subjected to full and frequent trial by others. We must add a caution to those desirous of using it, that care be taken to employ a pure salt, which is not always the state in which it is seen in this country. It is commonly rendered impure by an admixture of resinoid matter, which gives it a faint-yellow or brown colour, and which we have some suspicion may be partly the cause of the unpleasant effects which have occasionally been observed to result from it.

The next subject, the effects of *Veratria* and *Colchicum*, we shall pass over cursorily, as Dr Bardsley's experience of their therapeutic operation is unfavourable. *Veratria*, the alkaloid of the Colchicum and White Hellebore, he finds to be a drastic cathartic, and as such not more useful in the treatment of diseases than elaterium, croton oil, and such other substances. His experiments with the colchicum present no particular novelty. His observation of its effects has led him to attach less value to it than some late writers have done; and our own experience agrees very closely with his.

In the subsequent article some valuable information is supplied as to the virtues of Iodine in the various diseases, for the cure of which it has been resorted to of late years. The following is a summary:—

In *Goître* he has used it to a considerable extent, but not with the great success which some prior observers have obtained. Of thirty cases nine only were cured, and four relieved. Of the patients who were cured, the greater number had laboured under the disease between twelve months and two years. Some of those who experienced no relief continued to use the hydriodate of potass for three years. *Dr Bardsley* never observed any bad effect result in such cases. In *Scrofulous tumours* he has frequently employed it, and with very considerable success. In *Palsy* it was not of the slightest benefit. In *Chorea* it was very rarely of any service. In *Ascites* connected with apparent enlargement of the liver, he considers it a valuable remedy; and several cases of this kind are related where it appears to have been eminently serviceable. He employed it both internally in the form of solution, and externally by way of friction in the form of ointment.

Three other short articles conclude the author's observations, and experiments on New Remedies. These treat of *Cinchonia* and *Quina* as remedies for intermittent fever, of *Gentianin* as a stomachic, and of *Emetin* as an emetic.

His remarks on Cinchonia are interesting, as they tend to show that this alkaloid is hardly inferior to Quina as a remedy for ague. Nine cases are related in which a cure was accomplished with facility by means of the former substance. Four were tertians of six weeks, two months, and eighteen months continuance; four quotidians of three, six, or twelve months duration, and one a quartan which had continued some months. From three to five grains were administered every fourth hour. Most of the patients had three mild paroxysms after the treatment was commenced; one had twelve returns. Two cases of tertian ague resisted the cinchonia, and subsequently yielded to arsenic. The *Sulphate* of *Quina* has proved in *Dr Bardsley's* hands as effectual a remedy for the same disease as in the hands of all previous practitioners. Twenty-five cases are given where it was used; twenty were cured, four relieved, and one experienced no benefit. He takes occasion to remark,—and in this we are led from our own experience to think he is correct,—that the dose of sulphate of quina usually given is unnecessarily large; three grains every four, five, or six hours he finds amply sufficient.—He has used the Quina also in other diseases with good effect, such as in infantile remittent fever, in the ulceration of the cornea which succeeds the purulent ophthalmia of children, in dyspepsia, chorea, malignant small-pox, and neuralgia.

The *Gentianin*, in the form of pill, given in the dose of one grain twice or thrice daily, proved useful in some instances of dyspepsia, attended with irritability of the stomach, in consequence of which bulky medicines were inadmissible. But it is not likely to supersede the infusion and tincture of gentian, as it does not tend, like these preparations, to keep the bowels open.

Emetin, the emetic principle of Ipecacuan, appears a good expectorant in frequent doses of a quarter of a grain united with opium, a mild diaphoretic in double that quantity, and a good emetic in the dose of five grains dissolved in rose-water. But, except that its slight taste and odour enable the physician to administer it without difficulty to children, it does not appear to possess any material advantage over the crude drug.

The remainder of *Dr Bardsley's* work consists of hospital reports on the *Treatment of Chorea*, the *Treatment of Diabetes*, and the employment of *Sulphureous Fumigations* in rheumatism, and diseases of the skin. We must refer the reader to the work itself on these subjects, as it is not possible to convey in an analysis an adequate idea of the extent of information accumulated by the author.

The general result of his investigations into the treatment of *Chorea* is, that the disease is generally cured by any of the modes of treatment which have at different times been proposed,—that the present prevalent practice of treating it by the liberal administration of cathartics, according to the plan recommended by *Dr Hamilton Senior* of this city, does not so frequently or so rapidly succeed as he has supposed, and that on the whole the most certain and expeditious plan is to combine that system with the employment of antispasmodics, more especially musk and assafœtida. The chapter on this disease is concluded by two illustrative tables. One contains the particulars of twenty cases treated by purgatives only, from which it appears that fourteen were cured, and that the average duration of the treatment was five weeks,—that four were relieved after seven weeks and a-half of treatment, and were subsequently cured by antispasmodics,—and that two obtained no relief after thirteen weeks of treatment, but were also subsequently cured by the same alteration. The other table comprehends twenty cases treated by purgatives and antispasmodics combined; and from the data here given, it follows, that all were cured,—that eighteen got well in three weeks and a quarter on an average, and several of these in less than a fortnight,—and that the two remaining cases recovered in ten weeks.

The principal object of the chapter on *Diabetes* is to point out the advantage to be derived from combining an animal regimen with opium and the warm-bath, and several cases are related which show that this method is well deserving of farther trial. Five cases are related.—The first patient after four months of treatment, increased in weight from 102 to 120½ pounds, while the urine, from being twenty-six pints daily, with a density of 1040, became two pints and a half at 1019, and natural both in taste and smell. This patient was dismissed in October 1825, and "has ever since enjoyed a tolerable state of health." —The second patient was three months under treatment. He increased in weight from 119 to 131 pounds, and the urine was changed from twenty pints of the density 1039, to two pints of the density 1026, and of very slight sweetness.—The third was under treatment four months. In that time his weight increased from 131 to 149 pounds, and the quantity and density of the urine decreased from fifteen pints at 1041 to three pints and a-half at 1029, with a faint sweetness remaining.—The fourth was also four months under treatment. Her weight increased from 102 to 109 pounds, and the quantity and density of the urine decreased from fourteen pints at 1046 to two pints at 1030, and it was at the close nearly natural in external qualities.—The fifth after being three months under treatment increased in weight from 126 to 138 pounds, and the urine from being thirty-three pints in quantity, and 1044 in density, became two pints only, 1027 in density, and but slightly sweet. These results exhibit a rapid and material amendment.—We must remark, by the way, that the author has adopted an extraordinary standard for the density of healthy urine, which, on the authority of *Dr Elliotson*, he takes as varying from 1010 to 1018. If *Dr Bardsley* will trust to his own authority in this matter, which he can very easily do, we have no doubt he will conclude, as we have done from many experiments, that chemists are right in fixing it between 1024 and 1031.

The report which follows, *on the Efficacy of Sulphureous Fumigations in some diseases of the Skin, and in Chronic Rheumatism*, is well deserving of a much more detailed notice than it is in our power to give. But it is already in so abbreviated a form that we can only state the general heads.—The managers of the Manchester Infirmary having erected sulphureous fumigation baths, on the principle of *Mr Wallace* of Dublin, our author and his colleagues have made frequent use of them, and appear quite agreed as to the extensive advantages to be derived from them in many diseases, but chiefly in diseases of the skin and chronic rheumatism.—The diseases of the skin which have benefited most from the sulphureous baths are scabies, impetigo, porrigo, prurigo, lepra, psoriasis, pityriasis, ichthyosis and pompholyx. In particular, *Dr Bardsley* conceives that scaly diseases derive much more benefit from them than is generally supposed to be derived from warm baths. Of forty cases of lepra twenty-nine were cured, and the remaining eleven were much relieved. Of twenty cases of psoriasis, some of which were of very long standing, fourteen were cured, five relieved, and one discontinued the use of the bath. A case of diabetes was materially improved by the same treatment. In three months the weight of the body increased from 118 to 138 pounds; the quantity of the urine fell from twenty-eight to two pints, and its density from 1038 to 1010; and six months afterwards the man continued to enjoy good health. —Many cases of chronic rheumatism derived very great advantage from the sulphureous baths. Of forty cases, thirty were cured, eight relieved, and two discontinued the baths. Of the cases which were cured many were of two years' standing, and a few had existed for three or even four years.

Here we conclude what we have to say of *Dr Bardsley's* labours. We have derived unmixed gratification from the perusal of his researches, and we are sure that the reader, who will turn from the present analysis to the original, cannot fail to derive from every section of it, a great deal of information of the most purely practical nature. We repeat, that, for the sake of our profession, we long to see many specimens of the same kind. Zeal, diligence, prudence, and accuracy of observation, with proper opportunities, are all the conditions required for one who would desire to undertake similar inquiries.

ART. VI.—1. *Elements of General Anatomy, containing an Outline of the Organization of the Human Body.* By R. D. GRAINGER, Lecturer on Anatomy and Physiology. London, 1829. 8vo. Pp. 526.

2. *A Manual of General Anatomy, or a Concise Description of the Primitive Tissues and Systems which compose the Organs in Man.* By A. L. J. BAYLE, D. M. P., &c. &c. &c. and H. HOLLARD, D. M. P., &c. &c. Translated from the French by HENRY STORER. London, 1829. 12mo. Pp. 318.

THAT the study of general anatomy is attracting more attention than it heretofore did, appears from the publication of these and similar works on the subject which have recently issued from the English press. The student is not now compelled, as formerly, to have recourse to Bichat and Beclard only, but has in his power several vernacular works, among which he may select the one which he thinks most deserving of confidence, and most suitable to his views.

The works now before us, though similar in the general character of treating of the same subject, differ, nevertheless, in various points in internal arrangement and composition.

The work of Mr Grainger is the larger, and, upon the whole, the more complete of the two. Though its object is general anatomy, a large proportion of general physiological matter is also introduced; and the author endeavours, by not only describing the structure, but exhibiting sketches of the properties and functions of the animal tissues, to communicate an outline of the organization and living properties of the human body in the state of health.

The French authors aspire at a more comprehensive plan. Though the manual is on a smaller scale, it embraces the anatomy of the elementary tissues, with short statements of their physical and physiological properties, not only in the healthy state, but in that of disease. It is easy to see that, within the compass of a duodecimo of little more than 300 pages, such a plan must be executed only in a very cursory and superficial manner. Though this is indeed the case, the work is by no means either contemptible or useless; and we believe it may answer the purpose of giving the student, when just commencing the study, a good idea of the nature and objects of general anatomy.

In arrangement, these two works differ as much as in general character. That of Mr Grainger is formed throughout in close imitation of the work of Beclard, which we introduced in a former number to the notice of our readers.* Like that author he begins with a physiological introduction, consisting of nine sections, in which are discussed the following topics; the distinction of inorganic and organic bodies, the difference between vegetables and animals, the characters of animal organization, general remarks on the human body, the physical and chemical characters of the fluids, the characters of the solids, the functions, the developement of organization, and lastly, of death and its consequences. Whoever compares this arrangement of subjects with that of the introduction to the work of Beclard, will find, that though the latter is divided into two sections, which are again subdivided, they are in all essential characters the same.

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* Vol. xxii. p. 403.

In the subsequent part of the volume, the arrangement is altogether so similar, and it differs in points so trivial, that it may be said to be that of Beclard, scarcely modified. This will appear by glancing at the following comparative tables.

Arrangement of M. Beclard.	Arrangement of Mr Grainger.
Chapitre I. Des tissus cellulaire et adipeux.	Chapter I. Of the cellular and adipose tissues.
Premiere section. Du tissu cellulaire.	Part I. Of the cellular tissue.
Seconde section. Du tissu adipeux.	Part II. Of the adipose tissue.
Chapitre II. Des membranes sereuses.	Chapter II. Of the serous membranes.
Premiere section. Des membranes sereuses en general.	Part I. Of the serous membranes in general.
Seconde section. Art. 1. Des bourses synoviales sous-cutanées.	Part II. Section 1. Of the subcutaneous bursæ mucosæ.
2. Des membranes synoviales des tendons.	Section 2. Of the bursæ mucosæ of the tendons.
3. Des capsules synoviales articulaires.	Sections 3 and 4. Of the synovial articular capsules, and the synovia.
4. Des membranes sereuses splanchniques.	Part III. Of the splanchnic serous membranes.
Chapitre III. Des membranes tegumentaires.	Chapter III. Of the cutaneous system.
Premiere section. Des membranes tegumentaires en general.	Part I. Of the cutaneous system in general.
Seconde section. De la membrane muqueuse.	Part II. Of the skin.
Troisieme section. De la peau.	Part III. Of the mucous membranes.
Chapitre IV. Du systeme vasculaire.	Chapter IV. Of the vascular system.
Premiere section. Art. 1. Des vaisseaux en general.	Part I. Section 1. Of the vessels in general.
2. Des terminaisons des vaisseaux.	Section 2. Organization. Section 3. Developement.
1. Des vaisseaux capillaires.	Part IV. Section 1. Of the capillary vessels.
2. Du tissu erectile.	Section 3. Of the erectile tissue.
3. Des ganglions vasculaires.	
Seconde section. Des arteres.	Part II. Of the arteries.
Troisieme section. Des veines.	Part III. Of the veins.
Quatrieme section. Du systeme lymphatique.	Part V. Of the lymphatic system.
Chapitre V. Des glandes.	Chapter V. Of the glandular system.
Chapitre VI. Du tissu ligamenteux.	Chapter VII. Of the fibrous system.
Premiere section. Du tissu ligamenteux en general.	Section 1. Of the fibrous organs in general.
Seconde section. Des organes ligamenteux en particulier.	
Art. 1. Des ligamens.	Section 4. Fibrous envelopes.
2. Des Tendons.	Of the periosteum.
3. Des enveloppes ligamenteuses.	
1. Des enveloppes des muscles.	Aponeuroses.
2. Des gaines des tendons.	Sheaths of tendons.
3. Du perioste.	

4. Des enveloppes fibreuses du systeme nerveux.	
5. Des membranes fibreuses composées.	
6. Des capsules fibreuses de quelques organes.	Fibrous envelopes of certain organs.
	Section 5. Of the ligaments.
	Section 6. Of the tendons.
Troisieme section. Du tissu fibro-cartilagineux.	Chapter VIII. Of the fibro-cartilaginous system.
Chapitre VII. Des cartilages.	Chapter VI. Of the cartilaginous system.
Chapitre VIII. Du systeme osseux.	Chapter IX. Of the osseous system.
Premiere section. Des os.	Part I. Section 1. Number of the bones.
Seconde section. Des articulations.	Section 2. Articulations.
Troisieme section. Du squelette.	
Chapitre IX. Du systeme musculaire.	Chapter X. Of the muscular system.
Chapitre X. Du systeme nerveux.	Chapter X. Of the nervous system.
Premiere section. Du systeme nerveux en general.	Part I. Of the nervous system in general.
Seconde section. Des nerfs.	Part II. Of the cerebro-spinal system.
Troisieme section. Des ganglions et du nerf sympathique.	Part III. Of the ganglions.
	Section 2. Of the great sympathetic nerve.

Thus it may be perceived, that with a few slight changes in the manner of subdividing the sections, and with some few additions and physiological commentaries, the arrangement of the present work is entirely that of M. Beclard. Even in the subordinate details of the treatise, the work of the French author has been very closely followed; and in the physiological discussions, where it has been set aside, Richerand is the great and general model on which Mr Grainger moulds his physiological doctrines.

In the composition of the work the same system of imitation may be recognized. Though Mr Grainger occasionally introduces matter from English authorities, and sometimes from the recent inquirers; though some of his descriptions are derived from Meckel, and occasionally he adduces the views or facts of Dr Gordon, the work of Beclard is the great source from which most of his descriptions may be traced. Thus the sixth section on the solids is occasionally translated, occasionally modified from the part of Beclard's introduction entitled *Des organes*, at 93, § 81; and the chief novelty of this section is the introduction of the transverse lines, recognized in muscular fibre by Dr Hodgkin and Mr Lister. The seventh section on the functions, corresponds to that of Beclard *on organism;* and the eighth, on the developement and modification of organization, is the same in many respects as that of Beclard at p. 112, § 103;—expanded, indeed, somewhat by the introduction of an account of the temperaments from Richerand. The ninth and last section, on death and its consequences, is also formed entirely on the model of the concluding article of Beclard *De la mort et du cadavre*, with all the essential ideas and distinctions derived from that work. Thus the account of putrefaction by the two authors is very similar:—"La putréfaction est un mouvement intestin, inverse de l'action organique, qui s'établit dans le cadavre, détruit toutes les combinaisons qui s'étaient formées par l'action vitale, en sépare les molécules, les ramène à un état plus simple de composition, les réduit en gas, en vapeurs, en putrilage, en terreau, et les rend ainsi à la masse générale des corps inertes."

"Putrefaction is an intestinal and spontaneous movement, the inverse of the organic action, which is established in a body totally deprived of life; which separates the particles of the organic textures; reduces them into a more simple state of composition, and finally converts them into gas, into pulp, into earth, and so returns their elements to the general mass of inert bodies.—Pp. 104–5.

"Le sang et les autres humeurs transsudent à travers leurs réservoirs, et imprègnent de leur couleur et de leur odeur les parois et les parties environnantes; de là la coloration des veines et du tissu cellulaire environnant en rouge, les taches imprimées à l'estomac et aux intestins par le foie, la rate, la vésicule biliaire, les infiltrations séro-sanguinolentes dans le tissu cellulaire et les membranes séreuses, leur coloration en rose, en rouge, en brun, et la coloration des parois de l'abdomen en une teinte bleuâtre ou verdâtre. Les humeurs de l'œil transsudent, d'où l'affaissement de la cornée, et, en se mêlant avec les corpuscules qui voltigent dans l'œil, forment un enduit terne."—P. 129.

"Thus the blood, escaping into the cellular membrane, allows the course of the large veins, and occasionally of the smaller branches, to be traced upon the surface of the body by so many dark or greenish streaks. In the same manner the bile of the gall-bladder tinges the duodenum and other parts; and the stomach is often marked by the liver and spleen. The cellular tissue and serous membranes are infiltrated by a sero-sanguineous fluid, giving to those parts a reddish or brownish colour; whilst the walls of the abdomen are of a bluish or greenish hue. The redness which is caused after death by the transudation of blood, is very liable to be mistaken for inflammation. This fact should be impressed on the mind of every medical man, on account of its importance both in pathology and medical jurisprudence. The humours of the eye become opaque, and, transuding, produce relaxation of the cornea."—P. 106.

In the subsequent part of the work, more expressly devoted to the anatomical history of the elementary tissues, both in the arrangement and in the descriptive details, the same plan of imitation may be recognized. The distinctions, the nomenclature, and the language of the foreign author are adopted; and, with the exception of some partial additions from more recent observers, and the physiological commentaries, little novelty can be observed.

These points of resemblance show that this work is by no means original, either in plan or composition. It may indeed be said, that Mr Grainger could not have adopted a better model than the work of Beclard, which exhibits not only the most learned and best view of the facts and doctrines of general anatomy, but is justly distinguished for assigning to each author his own share of merit. It need not, however, be matter of surprise that we regard it as unnecessary to go into the detailed examination of a performance so constructed, and consisting chiefly of compilations from works which we have already noticed in the course of our critical duties. It is indeed true that Mr Grainger does not reproduce the whole of the treatise of M. Beclard, nor does his work contain nothing but what is found in the former; but if all that is derived from this source were removed, the originality and novelty of the remainder would be incapable of affording materials for any larger notice than the present.

Before concluding, however, we may say, that the work is useful; and though there is too much physiological matter, and that by no means of the most valuable description, the anatomical reader will peruse it with interest and advantage. The description of the teguments and the developement of the vascular system, which the author evidently derives from Meckel, is reasonably accurate; but Mr Grainger seems to take his information too much at second-hand to enable him to speak with much confidence. He is also occasionally inaccurate in the use of terms. *Dermis* is neither Greek, Latin, nor English.

Of the second work, we have little more to say, than that it exhibits a short, but upon the whole correct, view of the anatomical constitution and pathological changes of the different textures. The work is divided into ten chapters, in which are successively considered, the anatomical characters, physical and physiological properties, and morbid changes of the textures and systems in the following order:—1*st*, The cellular system, including cellular and adipose tissue; 2*d*, The vascular system, including, after a few general considerations, the arteries, capillaries, veins, and lymphatics, under the two heads of tubes and glands; 3*d*, The serous system in three sections, on general considerations, the splanchnic serous membranes, and the synovial membranes; 4*th*, The fibrous system in three divisions; —the first, the fibrous system proper, including ligaments, tendons, aponeurotic expansions, periosteum, perichondrium, fibrous envelopes, and fibro-serous and fibro-mucous membranes; the second, the yellow or elastic fibrous system; and the third, the fibro-cartilaginous system; 5*th*, the cartilaginous system, including after generalities, the articulating and the perichondrial cartilages; *6th*, The osseous system, or bones in all their varieties: *7th*, The nervous system, in two divisions, the nervous centres, including the brain and ganglia, and the nerves, cerebro-spinal and gangliar; *8th*, The tegumentary system, including the skin and mucous membranes, and their appendages, the hair, nails, and teeth; *9th*, The glandular system; and *10th*, The muscular system.

With the view of communicating some idea of the sources of information on each of these topics, to the end of each chapter is attached a list of works treating more or less fully and accurately of the subject either in health or disease. These lists, however, are not well arranged, and show little acquaintance with the subject. The best arrangements for such lists is the chronological, which indicates the progressive advancement of knowledge; yet this is so much violated, that the reader is recommended, first, to a book published in 1824, and then to one published in 1780 or 1760. Some of the best works also are omitted; for instance, in the chapter on the lymphatics, neither Hewson, nor Sheldon, nor Mascagni, are mentioned; and, on the teeth, the only English work noticed is that of Fox. It is easy to see that a work of this kind is not well calculated for directing the student to the best sources of information.

In a short appendix an account is given of the accidental tissues, or new formations, in the usual order,—tubercles, scirrhus, encephaloid cancer, and melanosis. This also is imitated from Beclard; and the learned authors seem either not to know, or not to admit the existence of the yellow degeneration described under the name of *cirronosos*, (not *cirrosis* as it is sometimes made) by Lobstein.

Art. VII.—*Compendium of the Diseases of the Human Eye, with Practical Observations on their Treatment. To which is prefixed an Account of the Anatomy and Physiology of that Organ. Illustrated by Engravings.* By Alexander Watson, Esq. Fellow of the Royal College of Surgeons, Edinburgh, &c. 3d Edition. 1830.

In this edition the author has enlarged his work, and rendered it more complete, by prefixing to the former a view of the anatomy and physiology of the eye, and a sketch of the diseases of the eyelids and other appendages of the organ. In its present state, therefore, the work consists of three parts, the first devoted to the explanation of the structure and functions of the eye; the second, to the elucidation of the diseases which affect its appendages; and the third, to those which are proper to the eyeball and its constituent parts.

The description of the anatomical structure is very clearly delivered, and shows that Mr Watson has not satisfied himself with merely repeating the accounts found in the ordinary anatomical works, but derives his information from careful and repeated dissection of the parts which he undertakes to explain. In accounting for the delicate structure of parts which cannot be sensibly demonstrated, he adduces arguments derived either from sensible phenomena or the effects of diseases and injuries. On the interesting but problematical subject of the connection between the fifth pair and the optic nerve, he candidly admits that it is impossible to determine the point satisfactorily. In the third chapter of part first, on the physiology, Mr Watson gives a good general view of the facts regarding the properties of light, the process of vision, and the part which each of the transparent portions of the eye performs in this valuable function.

The second part, which, like the first, is new, is devoted to the diseases of the eyelids and other appendages of the organ; and of these Mr Watson gives a short clear exposition, which is obviously the result of personal acquaintance with the subject. On these points it cannot be expected that much novelty should be exhibited. But the reader will find the history and characters of the several diseases fairly stated, and the appropriate treatment delivered in a clear and practical manner. As examples of this, the sections on eversion and inversion of the eyelids may be especially mentioned. The fourth and fifth sections of chapter second, on inflammation and its effects on the lacrymal sac and duct, are also deserving of notice; and the various modes of treatment applicable to the different degrees and stages of these maladies are explained ably and intelligibly.

The third part of the work, on the diseases of the eyeball, is improved by a few additions and changes in the different chapters of which it consists. This, indeed, constituted the work as it formerly appeared when last noticed in this Journal; and we are happy to say, that the opinion which we then expressed as to the utility of the treatise, we see no reason to retract, or even to modify in regard to its present form. It is foreign to our purpose, and inconsistent with the limits assigned to this notice, to enter minutely into the details of the work,—a task which further is rendered more difficult by its abridged form and elementary character. But we cannot close these observations without adverting to the value of the descriptive and practical parts on the diseases of the cornea, on those of the iris, and those of the retina. The description of inflammation of the proper cornea, of that of the iris, and the remarks on the diagnosis of this from ordinary ophthalmia, merit particular attention. The account of iritis, indeed, is full and minute, and may be recommended as the best extant for enabling the surgeon to recognize this dangerous and often unmanageable form of inflammation. The pathological principles are evidently the result of much accurate observation; and we believe Mr Watson is perfectly justified in stating, that it is impossible to distinguish from the mere appearance and symptoms, idiopathic iritis from the symptomatic forms resulting from syphilis, gout, and rheumatism. *Glaucoma,* Mr Watson ascribes to chronic inflammation of the retina, or the parts immediately connected with it; and it is interesting to remark, that Mr Watson is the first, so far as we remember, who has on rational principles undertaken to explain the nature of this disease, and directed the mind of the surgeon to the mode of treatment most likely to be efficacious.

In his account of the diseases incident to the lens, and the effect of injuries on that part of the organ of vision, Mr Watson directs the attention of the reader to the principles on which the operations for cataract are understood to act. The subsequent description of the different kinds of cataract is accurate; and the mode in which the author distinguishes the several varieties of capsular and lenticular opacity is the most satisfactory with which we are acquainted.

In illustration of the different forms of disease, the delineations given by Mr Watson are highly valuable; and we are happy to say, that to this work the student may apply with the certainty of obtaining satisfactory information on any disease of the organs of vision likely to come under notice.

ART. VIII.—1. *De Nervi Sympathetici Humani Fabrici, usu, et morbis, Commentatio Anatomico-physiologico-pathologica, Tabulis Aeneis et Lithographicis Illustrata.* Auctore JOH. FRED. LOBSTEIN, Med. Clin. et Anat. Pathol. in Facultate Medici Argentoratensi Professore, &c. Parisiis. 1823. 4to. PARS PATHOLOGICA.

2. *A Treatise on Neuralgic Diseases dependent upon irritation of the spinal marrow and ganglia of the Sympathetic Nerve.* By THOMAS PRIDGIN TEALE, Member of the Royal College of Surgeons in London, &c. &c. London, 1829. 8vo. Pp. 120.

THE influence of the nervous system in producing disease has been generally recognized since the days of Willis; nor did the agency of the great sympathetic nerve escape the sagacity of the same ingenious observer, who, after a minute anatomical description of the distribution of this important nervous chord, ascribes to derangement of its properties many hysterical and hypochondriacal complaints and other anomalous nervous maladies. * Thus amidst the confusion and mysticism of the psychological hypothesis of animal spirits, we find in his account of this nerve the first intelligible exposition of the associated actions of the digestive organs, of the phenomena of *globus hystericus* and spasmodic colic, and the explanation of the vomiting and colic pains in *nephritis.* That Willis, in the estimation of his contemporaries, over-rated the influence of the sympathetic chord, may be concluded from the silence observed on this part of his theory for near a century. It was not till some time after the middle of last century, when Johnstone in this country, and Unzer in Germany, directed the attention of physiologists and pathologists to the importance of the ganglions in the system, that the chord by which they are connected had some prospect of performing an important part in the theory and practice of the art of healing.

The hypothetical views of Johnstone and Unzer, though adopted with more or less modification by Metzger, Hufeland, Prochaska, Sue, and Harless, were very generally neglected by pathologists, till the ingenuity of Bichat succeeded in procuring them more general attention. The speculations with which this author expanded the hypothesis of Johnstone would have been of little moment, had not his personal observation on the influence of the ganglions in health and disease impressed them with interest to the pathologist. Adverting to the changes which the texture of the ganglions may undergo, and the rarity of their lesion in cancerous affections of the stomach, when the contiguous tissues are much indurated, he continues;—" On another occasion, I found in the person of a man brought to the Hotel-Dieu for periodical mania, the semilunar ganglion the size of a small nut, with a slight cartilaginous knot in its centre. Some physicians think, (he here alludes to Willis,) and I also suspect that hysterical paroxysms, which begin with constriction of the epigastrium, in which the patient feels a ball ascending to the throat, may depend on certain lesions of the semilunar ganglions, of the solar plexus, and of the communications which proceed from ganglion to ganglion to the neck. Nevertheless, two subjects whom I lately inspected presented no similar alteration, though during life the individuals had been frequently attacked with these paroxysms; but these might evidently proceed from the epigastric ganglions and plexuses, without the latter being affected in structure, precisely as numerous affections of the brain leave after them no trace in the texture of the organ. This point merits particular examination."

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* Nervorum Descriptio et Usus, cap. 25, 26, 27, in Operibus Thomæ Willis. Amstelædami, 1682.

Shortly after, when considering the physiological properties of the ganglions, his propensity to generalization breaks out in the following train of remarks:—"The ignorance in which we are on the diseases seated in the ganglions, the remoteness of these organs from all external agents of excitement are reasons why we have no precise facts on their sympathies. I think it very probable, nevertheless, that these sympathies perform an actual part in hysterical affections, in certain varieties of epilepsy, the accessions of which begin like those of hysteria, with a painful sensation in the epigastric region, in that numerous train of disorders named *nervous*, and which the vulgar confound under the denomination of vapours. One of the most important objects of research in the *Neuroses*, is to distinguish those which are particularly seated in the cerebral nervous system from those which more especially affect the ganglions. Place on the one hand palsy, hemiplegy, infantile convulsions, tetanus, catalepsy, apoplexy, most epilepsies, all the numerous accidents resulting from effusion, compressions of the brain from wounds of the head, neurotic diseases of vision, hearing, taste, smell, &c. and all the affections, the source of which is evidently in the head. On the other hand, place hysteria, hypochondriasis, melancholy, and the numerous class of affections in which the belly or the chest, but especially the former, appear to be the centre in which the complaint is seated; and you will recognize an essential difference in character and symptoms. I do not say that the latter family of *neuroses* affect the ganglions exclusively; for these affections are involved in too much obscurity to pronounce any positive opinion either on their seat or on their nature. Doubtless the secretory, circulating, and respiratory organs may be then particularly affected in their proper tissue independently of the nerves which they receive; but it is an interesting object of research, and there is too great difference between the one and the other order of affections to justify the idea that their primary seat is not different. It is difficult to believe that the system of the ganglions does not partake largely in the latter."

"What induces me to think that the difference of phenomena presented by the general order of *Neuroses*, is to be especially ascribed to difference betwen the cerebral and the gangliar nerves, is, that their phenomena in the state of health are very different. M. Hallé has justly remarked, that the pains experienced in parts supplied by gangliar nerves have a peculiar character, and that they do not resemble those felt in parts which receive cerebral nerves. Thus the painful sensation felt in the loins in affections of the womb, that resulting from the vinous injection into the *tunica vaginalis*, &c.—a sensation which I ascribe to the sympathetic influence exercised by the affected organ on the lumbar ganglions, pains of the intestines, &c. do not resemble external pains, but are deep-seated, and, as is popularly said, go to the heart. We know that some colics are essentially nervous, and entirely independent of local affection of the serous, mucous, and muscular systems of the intestines. These colics are manifestly seated in the nerves of the semilunar ganglions, which are distributed over the whole course of the abdominal arteries. They are genuine neuralgias of the nervous system of organic life; and these neuralgias have nothing in common with *tic douloureux*, sciatica, and the other neuralgic affections of the nervous system of animal life. In symptoms, progress, and duration, these affections are entirely different from each other." *

To these conjectures a more substantial shape has recently been given by John Frederic Lobstein, the professor of pathological anatomy at Strasbourg. This author, after an elaborate and accurate account of the anatomical structure and physiological properties and uses of the sympathetic chord, gives a most complete and extensive view of the maladies which he thinks may be traced to its derangements, either dynamic or organic. Among these we find not only *hypochondriasis*, *hysteria*, and colic, but mania and melancholy, hooping-cough, *angina pectoris*, night-mare, the preliminary anxiety of miliary fever, latent gout, the paroxysms of ague, the suddenly fatal effects of blows on the pit of the stomach, electricity and galvanism, and what are generally termed nervous shock, with a multitude of symptomatic disorders, popularly termed nervous, but affecting organs remote from those immediately under the influence of the nerve and its ramifications. Thus the megrim which affects hypochondriacal and hysterical patients, the *cephalœa* attending dyspeptic disorders and obstinate constipation, the vertigo, double vision, and *scotomia* attending worms, the watchfulness of dyspeptics, even the ophthalmies, amaurotic seizures, and toothachs of dyspeptics, are all to be ascribed to the agency of derangement in the state of the gangliar system and its nervous branches. And to the same cause are attributed the fatal effects of sudden joy or grief, the variety of apoplexy supposed to be nervous, and the anomalous assemblage of symptoms designated under the general epithet of *ataxia.*

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* Anatomie Generale, Tom. i. p. 227-229.

In this theory there may appear something rather too general and comprehensive for the actual facts of the case ; yet when we bear in mind the extensive connections of this extraordinary nervous chord, its numerous anastomotic communications, and the class of organs to which its branches are distributed, it is difficult to deny that it may perform the extensive and multifarious part assigned to it. On the other hand, it must be avowed that much of the theory is absolutely conjectural; and while part of it is merely probable, a good deal rests on no other evidence than that of simple possibility. M. Lobstein even does not attempt to deny this objection to his theory ; and admits, that of the affections of the sympathetic nerve, no sensible evidence can be obtained. In regard to one lesion, however, that of vascular injection or inflammation, he was more fortunate. In the body of a married female, subject from the age of puberty to spasmodic and hypochondriac complaints, and who, after being harassed by vomiting for three months during pregnancy, during which she complained of a sense of burning pain at the spinal column, causing incessant jactitation, was cut off by symptoms of most advanced marasmus, he found the brain and organs of the chest, the stomach, and the bowels, in a sound state, but the semilunar ganglion of an intense red colour, which was allowed by skilful spectators accustomed to anatomical inquiries, to indicate true and genuine inflammation, —an inference which the author regards as so much more just, that the red tint was scarcely abated after three days' immersion in cold water.

As the attention of Lobstein was, by the result of this case, particularly directed to the state of the semilunar ganglions and solar plexus, in persons cut off by diseases in which abdominal neuropathy was suspected, he soon after found, in the body of a girl of six, cut off by hooping-cough, terminating in spasmodic vomiting and convulsions, the left side of the solar plexus inflamed so decidedly, that, after several days' immersion in water, when the red colour was abated, a yellow tint, which was not observed in the right side, was left. In the brain he had sought unsuccessfully for morbid appearances, adequate to account for the phenomena. M. Lobstein further quotes, in illustration of the same views, the authority of Autenrieth, who, in the person of a girl also destroyed by hooping-cough, found the *nervus vagus* inflamed in its entire thoracic course, and the cardiac nerves, and the portion of the sympathetic contained within the chest, also morbidly altered. In other two cases communicated by **Aronssohn**, an able surgeon of the Strasbourg Hospital, inflammation of the semilunar ganglions was recognized, in connection with opisthotonos and inflammation of the villous coat of the intestines. Lastly, in a boy of 10, who died under the epigastric anxiety of repressed *miliaria*, he found part of the intercostal trunk included between the eighth and tenth ribs deeply inflamed, with injection of the ninth and tenth thoracic ganglions, and of two of the anastomotic twigs issuing from the costal nerves.

These facts, which constitute a much more direct species of evidence, are certainly entitled to the regard of the pathological inquirer. They are valuable in leading him to the investigation of a class of morbid phenomena which have no doubt been too much neglected, and which, though not capable of verification to the extent imagined by some of their warmest advocates, may, nevertheless, lead to results highly useful both in theory and practice.

The same, however, cannot be said of those morbid changes and appearances, which M. Lobstein recognizes in the nerves of organs already diseased. However important these may be as necroscopic facts, they are of no use as evidence regarding the influence of morbid conditions of the sympathetic chord, its ganglions, or its ramifications. We repeat, that it is only in cases in which trains of morbid phenomena terminating fatally, and shown by inspection to depend on no sensible lesion of the brain or spinal chord, the lungs, the heart, the stomach, or bowels, or any organ essential to life, that the circumstance of preternatural redness, unusual hardness or softness, enlargement or diminution of the ganglions, the plexuses, or any of their branches, can be admitted to be satisfactory evidence that disorder of the sympathetic chord was the cause of the complaints during life. Where the lesion is only dynamical, indeed, this rule is not applicable; but wherever it is organic, which is likely to be much more frequent, it holds true. For the reasons now assigned, we do not admit the observations adduced by M. Lobstein of redness or enlargement, softness or induration, of nervous chords going to or connected with, inflamed or hepatised or tuberculated lungs, or with aneurisms of the aortic arch, to be evidence of the primary morbid states of the nerves of the gangliar system.

The last author who has undertaken to cultivate this field of pathological inquiry is Mr Teale; and, though he has not improved it much, his work is by no means undeserving of attention. In imitation of Bichat, and a number of recent authors, he extends the term *Neuralgia* to all disorders of nerves, whether accompanied by pain or not, so that *Neuralgia*, according to this system, is made to denote in general any morbid state of a nerve, nervous chord, ganglion, or plexus. This innovation it is perhaps not worth while to dispute; at least we care not to enter into any discussion about it, otherwise it would be easy to show that the extension of the term is attended with the equivocal, perhaps the superior disadvantage of depriving the pathologist of a name to designate affections simply painful, and is indeed thereby transferred from being used to denote matter of fact to indicate what may prove to be hypothetical. This, however, we pass. The peculiarity of the views of Mr Teale consists in this, that he regards several anomalous affections and certain trains of morbid phenomena, which appear to reside in one class of organs, and to which, therefore, they are commonly referred,—as dependent on irritation either in the spinal chord or its nerves, or in the ganglions, plexuses, or branches of the sympathetic nerve.

In the first part of this hypothesis, Mr Teale is anticipated by Dr Whitlock Nicholl, who published in 1821 several interesting views of the effects of cerebral and spinal irritation;—and more recently by Mr Player of Malmesbury, by Dr Brown of Glasgow, and by Dr Darwall of Birmingham. To the three latter authors, Mr Teale renders ample justice. The work of Dr Nicholl he does not appear to have seen; but we take this opportunity of saying, that it contains a number of ingenious views, which ought to have procured it more general notice than it appears to have received in this country. *

The views of the present author will be best understood by the following extracts:—

> " It is now pretty generally admitted as a pathological axiom, that disease of the larger nervous masses, as the brain and spinal marrow, is not so much evinced by phenomena in the immediate seat of disease, as in those more remote parts to which the nerves arising from the diseased portion are distributed. In the more severe forms of disease, this principle is readily admitted and recognized. When, for instance, one-half of the body shall have lost its sensibility, and the corresponding muscles their power of action, the skin and the muscles are not regarded as the seat of disease, but the *brain* is immediately referred to. In the slighter forms of disease of the brain and spinal marrow, such as do not completely obliterate, but merely impair or pervert the functions of the nerves—such as do not paralyze the sentient and muscular powers of the part, but produce weakness, tremors, spasms, &c. in the muscular system, and numbness, pricklings, pains, and other morbid feelings in the nerves of sensation, this important

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* On Disordered States of the Cerebral Structures occurring in infants. By Whitlock Nicholl, M. D. London, 1821.

principle, which as strictly obtains as in the former instance, is too often entirely overlooked; and a numerous class of complaints, of very frequent occurrence, are regarded as nervous or spasmodic diseases of the part affected, instead of being considered as actual diseases of that portion of the brain or spinal marrow from which the nerves of the part are derived." P. 3.

The history of the symptoms of the complaint which, according to this hypothesis, is irritation of the spinal chord, deserves attention; but as we must pass from this to the subject of irritation of the sympathetic, we must be satisfied with recommending it, as well as the account of the remedial measures, to the particular regard of our readers.

The same principles as those now stated regarding the pathological influence of the spinal chord, Mr Teale thinks are applicable to that of the great sympathetic and gangliar nerves.

"The same pathological principle is, I believe, equally applicable to the sympathetic system of nerves; and although it may be difficult to establish this opinion by actual experiment, yet I think it may be rested upon a well-grounded analogy, which will justify us in regarding the nervous masses of the ganglionic system as bearing the same relation to the nerves derived from them, as the large nervous masses of the cerebro-spinal system bear to their respective nerves. Hence, many nervous affections of the viscera ought not to be considered as diseases of the viscera themselves, but as symptomatic of disease in those particular ganglia whence their nerves are derived." Pp. 3–4.

The difficulty of establishing the truth of this hypothesis on unequivocal and direct evidence Mr Teale admits; but expresses his opinion at the same time that, from the phenomena of the nervous system, and the laws by which they are regulated, there is a strong presumption that the inference is well founded. After some observations on the facts adduced to determine these laws, Mr Teale proceeds in the following manner.

"I must now refer to the pathological principle with which I commenced; namely, that disease of the nervous masses is not so much evinced by symptoms in the immediate seat of disease, as by phenomena exhibited in those remote parts to which the nerves arising from the diseased portion are distributed. Upon this principle those nervous diseases of the heart, lungs, and stomach, which have been shown to be more probably dependent upon the sympathetic than upon the cerebro-spinal system of nerves, *should not be regarded as diseases of the particular filaments distributed to these organs, but as diseases of the ganglia or masses from which the filaments are derived.*

"The *probability* that these diseases depend upon an affection of particular ganglia is still further corroborated by the fact that tenderness may generally be detected in that part of the spine which is contiguous to the particular ganglia. Thus, when the heart is affected, there is tenderness in the cervical vertebræ; when the stomach is affected, the tenderness is seated in the middle or lower dorsal portion of the spine. The result of treatment directed to these parts may be still further adduced in corroboration.

" In the absence of direct evidence from dissection, the precise nature of these affections of the spinal marrow and ganglia must to a certain degree remain conjectural. The collateral evidence, however, is of such a nature as to leave but little doubt of the disease being inflammatory.

" Throughout this treatise, I have in general used the term irritation in preference to inflammation, supposing that to many the former would be less objectionable during the present deficiency of *post mortem* evidence. I wish, however, to be understood as using the word irritation in the same sense as many continental writers have lately employed it, regarding irritation and inflammation as morbid states, differing from each other merely in degree, and exhibiting no distinct line of demarcation.

" The supposition that these diseases are inflammatory is greatly strengthened by a comparison of the symptoms which they present with those attendant upon the more severe forms of inflammation of the spinal marrow, which have proceeded to a fatal termination, and have left unequivocal traces of their existence. The symptoms of the former, on careful examination, will appear simply as slighter shades of the more severe affections. The skin, instead of being entirely deprived of feeling, has its sensibility merely perverted or impaired; and the muscles, instead of being paralysed, are simply enfeebled or affected with tremors or spasms. And if the symptoms which mark the *commencement* of the more severe diseases of the spinal marrow be carefully observed before they have reached their advanced state of developement, they will often be found precisely similar to and even identical with the signs which denote these milder forms of disease. There may also be observed every gradation of symptoms, from those which denote the mildest irritation of the spinal marrow to those which attend its complete disorganization. Hence it may be inferred that they are but slighter shades of the same disease—inflammation; which, however, may exist, as we observe in other organs, in so low a degree as to produce annoying functional disturbance without tending to actual disorganization; and seldom, unless aggravated by accidental circumstances, proceeding to a fatal termination.

" It might be urged in objection that inflammation could not exist for so long a period without producing disorganization of the affected parts. I would, however, ask, Do we not in the children of the poor repeatedly observe a chronic form of inflammation of the eyelids (ophthalmia tarsi,) which will continue for months or years without any disorganization or destruction of parts?

" The tenderness of the spine in that particular part, which the nervous symptoms would indicate as the seat of disease, may be considered as corroborative of the inflammatory nature of the disease. It might, however, be argued that the tenderness is not confined to the exact site of the spinal marrow and its membranes, but is more diffused, being perceived over the whole extent of the affected vertebræ; or it might be urged that the spinal marrow and its membranes are so securely protected by the vertebral column as not to be influenced by any moderate degree of pressure upon the vertebræ. To these objections I would reply that the affection most probably is not confined to the spinal marrow and its membranes, but that the surrounding tissues, as the ligaments, the cellular connecting medium, &c., are all involved in a state of phlogosis. This would account for the sensibility to pressure whenever the connecting ligaments of the vertebræ were thereby disturbed. When the symptoms would lead one to suspect that both the ganglia and the corresponding portion of the spinal marrow are affected, it is probable that the disease may have extended from one to the other through the medium of the connecting or anastomosing nervous branches." Pp. 64—67.

Upon the strength of these arguments, Mr Teale establishes *neuralgia* of the heart, indicated by palpitations, *neuralgia* of the stomach, indicated by various dyspeptic symptoms, *neuralgia* of the lungs, denoted by asthma and other derangements of their functions, *neuralgia* of the intestines, producing colicky pains, of the kidneys, of the bladder, and of the uterus, giving rise to various painful and anomalous sensations. The justice of these distinctions he corroborates by appealing to the authority of Lobstein, from whom also he quotes the necroscopic instances already mentioned; and upon the same grounds he condemns as erroneous the practice of referring to sympathetic irritation of the stomach and bowels, many neuralgic affections in children, and that of classing the neuralgic symptoms of adults under the head of diseases of the digestive organs and hysteria respectively. Mr Teale, upon similar principles, refers to neuralgia sundry affections of the extremities, of the scalp, of the thoracic and abdominal parietes, and, in short, of every texture and organ which is under the influence of the nervous system. Lastly, after a view of the phenomena and circumstances of *angina pectoris* in the principal recorded cases, Mr Teale does not hesitate to regard this affection, as neuralgia of some part of the spinal chord and the ganglia corresponding to the splanchnic nerves.

"On referring to the various cases of angina pectoris which have been recorded, we find that the disease, in its simplest form, consists in the affection of the lower dorsal portion of the spinal marrow, attended with constriction and tightness across the epigastrium; to this is most frequently added an affection of the corresponding thoracic ganglia of the sympathetic from which the great splanchnic nerves are derived, producing distension, flatulence, and pain in the stomach; frequently there also exists an affection of the cervical portion of the spinal marrow, causing the pains, and other morbid sensations, in the neck, breast, and upper extremities, particularly on the left side, and perhaps occasionally dyspnœa from impaired action of the diaphragm; and not unfrequently there exists a similar affection of the cervical ganglia of the sympathetic, producing, in addition to the former symptoms, palpitations and painful affections of the heart and lungs.

"This inflammatory affection of certain portions of the ganglionic system, and of the spinal marrow, is not unfrequently accompanied by various organic diseases of the heart itself, as ossification of the coronary arteries, of the valves, or of the aorta; softening, hypertrophy, or dilatation; producing, in addition to the symptoms peculiar to disease of the ganglia and spinal marrow, additional symptoms, peculiar to disease of the heart itself, as the more severe forms of palpitations, intermittent pulsations, obstructed circulation, and consequent lividity, anasarca, &c. From this combination we may easily comprehend why these affections so often prove suddenly fatal, and why organic diseases of the heart are so generally, though not universally, found on the dissection of those subjects who have died of angina, since the disease seldom proves fatal, unless accompanied with some organic disease of the heart. It is also possible, and even probable that the disordered state of the nerves of the heart, from an affection of the cervical ganglia, may predispose to the organic changes in that organ which have been so frequently observed.

"The paroxysmal exacerbations of the complaint depend upon undue accumulations of blood in the heart, which render it incapable of acting with sufficient energy to propel the blood with freedom, particularly when its muscular structure is in a state of atony or atrophy, from ossification of the coronary arteries, fatty degenerescence or softening; or when there exists any obstruction at the different apertures. And it is not improbable that, when the nerves of the heart are simply affected from disease of the ganglia and a consequent state of nervous palpitation without organic disease of the heart exists, that the muscular structure may be so weak and irritable as to be unable to act with that regularity and decision which is necessary for maintaining a regular and equable transmission of the blood. The causes which excite these paroxysms are such as accelerate the circulation or determine the blood more rapidly to the heart than it has the power of transmitting; as passions of the mind, exercise, and that state of the system which of late has been termed congestion, in which the blood ceases to circulate freely in the superficial parts of the body and in the extremities, and is accumulated in some of the internal organs; a state attended with coldness and pallor of the surface, and oppressed functions of those organs which are more particularly the seat of the congestion. From any of these causes blood may be accumulated in the heart and large vessels, and, when the muscular apparatus of this organ is impaired by any alteration of structure or by disease of its nerves, it may be unable, at least for a time, to restore an equable state of the circulation;

a sense of anxiety, of pain, or of fulness in the region of the heart, and an impression that life is actually on the point of being suspended, is the result. In this state, after ineffectual efforts to propel its contents, the heart, as it were, feels its inability, and sometimes yields to a temporary paralysis or syncope, and not unfrequently, its tone is so completely overpowered, that it remains for ever paralysed.

"The earlier writers applied the term angina pectoris only to the more severe forms of the affection, from which arose the opinion that it was a disease of extreme danger. Daily observation, however, abundantly testifies that it may exist in every degree of violence, from the slightest sense of oppression and flatulence to the inexpressible anxiety, constriction, and suffocation, which mark the more aggravated forms of the disease, particularly when they are combined with an organic affection of the heart. On this subject I am glad to find my views correspond with those of Laennec, who considers that in a slight degree it is a disease of very frequent occurrence.

"I have been induced to refer the various groups of symptoms which have been described as angina pectoris, to an affection of some portion or portions of the spinal marrow, and of the corresponding ganglia of the sympathetic, by the following considerations.

"1. The fact, as I have before observed, that most of the morbid phenomena exhibited in the extreme filaments of nerves, are seldom owing to disease in the nerves themselves, but to an affection of the nervous mass from which they are derived.

"2. The co-existence of pain on pressing some portion of the spine with the symptoms constituting angina pectoris; and the correspondence of the painful part of the spine with the particular symptoms which are present; namely, tenderness in the lower dorsal portion of the spine in conjunction with the stomach affection, constriction, &c. and tenderness in the cervical spine, with pains in the arms, breasts, and shoulders, and palpitations.

"3. The relief obtained by local antiphlogistic measures to the spine; for instance, to the lower dorsal portion when the stomach is affected, and there is constriction, and to the cervical portion when there is an affection of the arms and palpitations." Pp. 104—108.

In these views, though it is manifest to any one acquainted with the pathological doctrines which the best authorities have taught on nervous diseases, that there is little more than change of name, yet there is also some ingenuity; and it is some advantage that it directs the attention of the practitioner to useful and intelligible indications, and renders the mode of treatment something more pointed and specific than before. In its present state, however, the theory is little more than an ingenious hypothesis, which deserves attention, and the practical applications of which admit of fair trial, with reasonable hopes of ascertaining its actual utility. The remedial measures employed by Mr Teale, apparently with success, consist in local bleeding by means of leeches, as near the part of the spinal chord, the nerves, or ganglia supposed to be affected, as possible, followed in general by blisters, and accompanied with purgatives and due regulation of the diet.

ART. IX.—*A Treatise on Poisons, in relation to Medical Jurisprudence, Physiology, and the Practice of Physic.* By ROBERT CHRISTISON, M. D. Professor of Medical Jurisprudence and Police in the University of Edinburgh, &c.

THE second part of Dr Christison's work treats of individual poisons. He divides them into the irritant, narcotic, and narcotico-acrid. The irritant poisons comprehend both those which have a purely local irritating action, and likewise many which also act remotely, but whose prominent feature of action is the inflammation they excite wherever they are applied. We think that some advantage might have been derived from keeping these two orders of poisons distinct, as subdivisions of the same class. There is this striking difference between them, that with the same quantity the action of the one is proportioned to their degree of concentration, and that of the other to their dilution. We would instance sulphuric acid as an example of the former, and oxalic acid of the latter. The alkalies are merely caustic, acting by their chemical agency. Arsenic possesses a destructive power of a different nature. In short, we would separate the merely corrosive from the irritant poisons.

In regard to each class of poisons, Dr Christison premises to the history of individual substances referable to it, an account of the symptoms and of the morbid appearances produced by them, compared with those of natural diseases. This is a subject of the greatest importance, as in cases of sudden or rapid death the vulgar, and what is more dangerous, the less instructed members of our profession, are exceedingly apt to suspect the felonious administration of poison.

The arrangement which Dr Christison has adopted in treating of each individual poison is calculated to prevent the omission of any material fact, and to render consultation easy. The general heads are;—the tests of the various poisons; the action and symptoms produced in man; the morbid appearances caused by them; and the treatment of poisoning by them. To the discussion of each of these topics Dr Christison brings all the varied qualifications required; consummate skill in the most difficult department of chemical analysis, indefatigable industry in the collection of facts, a severe and critical judgment in estimating their relative value, and in deriving from them general conclusions, and personal experience, greater perhaps than has fallen to the lot of any individual in this country.

We cannot attempt to condense the valuable information contained in every part of this work, nor even to extract what may be justly considered as original. We must content ourselves with selecting a few observations which we have noted in the course of our perusal.

The external application of corrosive substances, with the intent of murdering or disfiguring any person, is now made a capital crime by the laws of Scotland. The proof of the crime rests sometimes on the detection of the poison in the articles of clothing touched by it, which will exercise the skill of the analytic chemist, as sometimes the corrosive thrown upon an individual for the purpose of injuring him already exists in the clothing as a part of the dye with which it is coloured. Thus, in the celebrated case of Euphemia Macmillan, Drs Christison and Turner procured from a corroded piece of the hat 4.3 grains of sulphate of baryta, and from a sound piece of the same size a scanty precipitate too small to be collected.

The poisonous action of oxalic acid is exceeedingly curious in many points of view, and has been greatly elucidated by the experiments of Professor Christison and Dr Coindet, for a detailed account of which we have only to refer our readers to the nineteenth volume of this Journal.

We pass over the alkalis, which are purely corrosive, and also their salts, of which only the oxalate and nitrate of potass sometimes give rise to accidents. The subject of arsenic, also, although perhaps the most interesting subject of toxicology, and one of which the discussion is most full, comprehensive, and satisfactory, we are enabled to omit, as the chief facts regarding it have already been repeatedly brought before the profession by Dr Christison, both in this Journal, and in the Medico-Chirurgical Transactions of this city.

We prefer going a little more into detail regarding mercury, which is the subject of a highly interesting article, comprehending all the known facts, clearly stated and well arranged. The forms in which mercury occasionally proves poisonous are the red precipitate, vermilion, calomel, corrosive sublimate, turbith mineral, and the cyanide. Of these, corrosive sublimate is the most important. Dr Christison explains the tests for detecting it in its solid state, and when dissolved. The former are of easy application. The detection of this poison when dissolved is also not difficult, unless when mixed with organic fluids and solids; and we shall extract Dr Christison's process for this purpose as a fair instance of the analytic skill displayed in this volume.

"On the whole, the following plan has appeared to me the most simple and most generally applicable. It is a double process, of which sometimes the first part, sometimes the second, sometimes both may be required. The first removes the corrosive sublimate undecomposed from the mixture, which may be accomplished when its proportion is not minute; the second, when the proportion of corrosive sublimate is too small to admit of being so removed, separates from the mixture metallic mercury; and the analyst will know which of the two to employ by using the protochloride of tin as a trial-test in the following manner.

"A fluid mixture being in the first instance made, if necessary, by dividing all soft solids into small fragments, and boiling the mass in distilled water, a small portion is to be filtered for the trial. If the protochloride of tin causes a pretty deep ash-gray or grayish-black colour, the first process will probably be successful; if the shade acquired is not deep, that process may be neglected, and the second put in practice at once.

First branch of the Process.—In order to remove the corrosive sublimate undecomposed, the mixture, without filtration, is to be agitated for a few minutes with about a fourth part of its volume of sulphuric ether; which possesses the property of abstracting the salt from its aqueous solution. On remaining at rest for half a minute or a little more, the etherial solution rises to the surface, and may then be removed with the pipette, (Fig. 6.) It is next to be filtered if requisite, evaporated to dryness, and the residue treated with boiling water; upon which a solution is procured that will present the properties formerly mentioned as belonging to corrosive sublimate in its dissolved state."

"*Second brauch of the Process.*—If the preceding method should fail, or shall have been judged inapplicable, the mixture is to be treated in the following manner. In the first place, all particles of seeds, leaves, and other fibrous matter of a vegetable nature are to be removed as carefully as possible. This being done, the mixture, without undergoing filtration, is to be treated with protochloride of tin as long as any precipitate or coagulum is formed. This precipitate, even if it contains but a very minute proportion of mercury, will have a slate-gray tint. It is to be collected, washed and drained on a filter; from which it is then to be removed without being dried; and care should be taken not to tear away with it any fibres of the paper, as these would obstruct the succeeding operations. The mercury exists in it in the metallic state for reasons formerly mentioned. (p. 272.)

"The precipitate is next to be boiled in a moderately strong solution of caustic potass contained in a glass flask, or still better in a smooth porcelain vessel glazed with porcelain; and the ebullition is to be continued till all the lumps disappear. The animal and vegetable matter will thus be dissolved; and on the solution being allowed to remain at rest, a heavy grayish-black powder will begin to fall down in a few seconds. This is chiefly metallic mercury, of which, indeed, globules may sometimes be discerned with the naked eye or with a small magnifier.

" In order to separate it, leave the solution at rest under a temperature a little short of ebullition for fifteen or twenty minutes. Fill up the vessel gently with hot water without disturbing the precipitate, so that a fatty matter, which rises to the surface in the case of most animal mixtures, may be skimmed off first with a spoon, and afterwards with filtering paper. Then withdraw the whole supernatant fluid, which is easily done on account of the great density of the black powder. Transfer the powder into a small glass tube, and wash it by the process of affusion and subsidence till the washings do not taste alkaline. Any fibrous matter which may have escaped notice at the commencement of the process, and any lumpy matter which may have escaped solution by the potass should now be picked out. The black powder is the only part which should be preserved. If the quantity of powder is very minute, an interval of twelve hours should be allowed for each subsidence, and the tube represented in Fig. 5 should be used.

" Lastly, the powder is to be removed, heated, and sublimed, as in the last stage of the process described in page 272, for detecting corrosive sublimate in a pure solution.

" The second branch of this process is very delicate. I have detected by it a quarter of a grain of corrosive sublimate mixed with two ounces of beef, or with five ounces of new milk, or porter, or tea made with a liberal allowance of cream and sugar. I have also detected a tenth part of a grain in four ounces of the last mixture, that is, in 19,200 times its weight." Pp. 280—282.

The next section treats of the mode of action of mercury, and the symptoms it excites in man. The result of the inquiry into this subject is, that corrosive sublimate causes, when swallowed, corrosion of the stomach, and in whatever way it finds entrance into the body, irritation of that organ, and of the rectum, inflammation of the lungs, depressed action, and perhaps also inflammation of the heart, oppression of the functions of the brain, and inflammation of the salivary glands. He next states the facts which render it probable that corrosive sublimate, before it produce its remote effects, must enter the blood, and explains the reasons why hitherto mercury has been in vain sought for in the fluids and secretions of those subjected to its action.

Dr Christison has considered the poisoning by corrosive sublimate as presenting three varieties; in one, the sole or leading symptoms are those of violent irritation of the alimentary canal; in the third, they are those of mercurial erethysm in some of its various forms; and in the second, these two classes of symptoms occur in succession. The secondary effects of mercury are very ably treated, and give occasion for many very curious inquiries. We have only room to quote Dr Christison's plan of treating this subject, and to refer our readers to the work itself for the very interesting details with which it is completed.

> " The secondary and chronic effects of mercury are multifarious enough in reality; but if credit were given to all that has been written and is still sometimes maintained on this subject, almost every disease in the Nosology might be enumerated under the present head; for there is hardly a disease of common occurrence which has not been imputed by one author or another to the direct or indirect operation of mercury. The present remarks, however, will be confined as much as possible to what is well ascertained, and bears on the medical evidence of poisoning with mercury, or is important in regard to medical police. With this view, salivation and its concomitants, the most usual of the secondary effects of mercury, will first be treated of. Some observations will then be made on the shaking palsy, or *tremblement mercuriel,* which is caused by mercury in those who work with it. And in conclusion, a short view will be taken of the other diseases which are more indirectly induced by this poison, as well as of some which have been ascribed to it on insufficient grounds. This being done, the mode of action of the mercurial poisons will be resumed, and a description given of their relative effects when introduced by different channels and in different chemical forms." Pp. 301—302.

The last section gives an account of the morbid appearances caused by mercury.

In the chapters upon the other metallic poisons, although not so extensive, all the useful facts are collected and well arranged. We cannot, however, resist the temptation of making known, as generally as our Journal is able to do, the very valuable and original information connected with the subject of lead, as an object of medical police.

> " Distilled water, deprived of its gases by ebullition, and excluded from contact with the air, has no action whatever on lead. If the water contains the customary gases in solution, the surface of the metal, freshly polished, becomes rapidly dull and white. But if the surface of the water be not at the same time exposed to the air, the action soon comes to a close. When the air, on the other hand, is allowed free access to the water, a white powder appears in a few minutes around the lead; and this goes on increasing till in the course of a few days there is formed a large quantity of white pearly scales, which partly float in the water, but are chiefly deposited on the bottom of the vessel. In twelve ounces of distilled water, contained in a shallow glass basin, loosely covered to exclude the dust, twelve brightly polished lead rods

weighing 340 grains, will lose two grains and a half in eight days; and the lead will then show evident marks of corrosion. The process of corrosion goes on so long as atmospheric air is allowed to play freely on the surface of the water, but gradually becomes less and less, provided the water be not occasionally shaken, to prevent the adhesion of the powder to the surface of the lead." Pp. 385—386.

The property which pure aërated water possesses of thus corroding lead, is variously affected by foreign ingredients which it may hold in solution. Of these, none are more remarkable in their action than the neutral salts, which all impair the corrosive powers of the water. Guyton-Morveau ascertained this with regard to sulphate of lime and the nitrates.

"Extending Guyton-Morveau's inquiries to other proportions of the same salts, and likewise to many other neutral salts, I have been led to the conclusion, that all of them without exception possess the power of impairing the action of distilled water on lead. At least I have found it to exist in the case of sulphates, muriates, carbonates, hydriodates, phosphates, nitrates, acetates, tartrates, arseniates,—which are all the neutral salts I have yet tried." P. 387.

"The preservative power depends on the acid, not on the base of the salt. The acetate, muriate, arseniate, and phosphate of soda differ exceedingly in power. On the other hand, the sulphates of soda, magnesia, lime, as well as the triple sulphate of alumina and potass, preserve as nearly as can be determined in the same proportion." P. 388.

"The general result of these experiments appears to be, that neutral salts in various, and for the most part minute, proportions, retard or prevent the corrosive action of water on lead,—allowing the carbonate to deposit itself slowly, and adhere with such firmness to the lead as not to be afterwards removeable by moderate agitation,—adding subsequently to this crust other insoluble salts of lead, the acids of which are derived from the neutral salts in solution,—and thus at length forming a permanent and impermeable skreen, through which the action of the water cannot any longer be carried on." P. 390.

According to these observations, Dr Christison explains many interesting facts which have been recorded, but for which we have not room. The various contaminations of articles of food and drink with lead, also deserve an attentive perusal.

Having finished the history of the metallic poisons, the vegetable acrids, animal poisons and mechanical irritants are next discussed with equal ability, though not at so much length. The account of alimentary articles rendered poisonous by modified putrefaction is exceedingly curious and important, as pointing out a source of sudden illness and death almost unknown in this country; but the able abstract by Dr Arrowsmith, of an inquiry by Dr Horn of Berlin, published in our last number, precludes the necessity of our entering into this subject more at large.

We must delay an account of the two remaining classes of poisons to a future number.

ART. X.—1. *A Practical Synopsis of Cutaneous diseases, according to the arrangement of Dr Willan, exhibiting a concise view of the diagnostic symptoms.* By THOMAS BATEMAN, M.D. &c. The seventh Edition, edited by ANTHONY TODD THOMSON, M. D. Professor of Materia Medica and Pharmacy in the University of London, &c. &c. Longman, &c. London, Pp. 460, 8vo. 1830.

2. *Atlas of Delineations of Cutaneous Eruptions; illustrative of the Descriptions in the Practical Synopsis of Cutaneous diseases of Dr Thomas Bateman.* By ANTHONY TODD THOMSON, M. D. &c. Longman, &c. London. 26 plates and 112 pages of descriptive letter-press. Small 4to. 1830.

IN the first of the works here announced the editor has followed a plan, which we believe has been introduced in the present instance for the first time into medical literature. The additions and corrections which the improvements in medical science have suggested are not added in the form of notes or in an appendix, but are embodied with the text by interpolation, the interpolated passages being distinguished by inverted commas. This is perhaps the only way in which a standard work of one period can continue to be the popular standard of succeeding periods; and if the additions are judiciously introduced, and the original author was cautious in laying down his general principles, so that such a method of editing is practicable, we conceive the plan is likely to be generally acceptable.

The present is a very favourable example of the kind. Dr Bateman's opinions and practice were founded on so purely practical a basis, and his exposition of them so simple, that, whatever his successors may have to add, they will find little to alter in his synopsis. Accordingly Dr Thomson's interpolations form with the original a continuous whole, in which, without the designating marks he has appended, the reader would not readily discover the joints. The additions, which are chiefly intended to facilitate the diagnosis of the different species and genera of cutaneous diseases, and to bring before the student the improvements which have taken place in their treatment, appear to us correct, judicious, and useful. If we were to search for a fault, we should be inclined to say, that he has too many remedies, and relies perhaps, too much on them.

The Atlas which accompanies this edition, but which is also sold separately, is intended to bring the study of cutaneous diseases by referring to coloured plates within the means of the general run of students. This object the author we hope will accomplish. The price of the Atlas is only three guineas, and its size so moderate, that the student can easily carry it with him to the bed-side of the patient, and in his hospital visits. The plates are for the most part reduced delineations of the greater work of Bateman, and their fidelity is on the whole striking. The colouring of some of the plates is perhaps too bright. But this fault it is exceedingly difficult to avoid. We have no hesitation in saying, that the work will be found of signal utility to the learner, by enabling him, after a little practice in making allowance for the inseparable defects of all delineations, to attach the correct names to almost any variety of cutaneous disease he may encounter.

PART III.

MEDICAL INTELLIGENCE.

PHYSIOLOGY.

On the Rapidity of the Circulation and of the Secretions. [*Journal des Progrès des Sciences et Institutions Medicales*, T. x. 20.]—*Dr Hering*, professor of the veterinary school at Stuttgardt, has recently made some very ingenious experiments to determine the rapidity with which the blood moves in the vessels, and is transformed into the various secreted fluids. He rejects entirely the mode by which *Hales*, *Keil*, and others after them endeavoured to discover the celerity of the movement of the blood, as founded on uncertain and unascertainable data; and had recourse to the test derived from the celerity with which particular substances may be proved by chemical analysis to pass from one part of the body to another.

His experiments were commonly performed on the horse, and his usual method consisted in introducing a solution of the prussiate or ferro-cyanate of potass into one of the jugular veins, and drawing blood at various short intervals afterwards, then killing the animal and examining without delay the various secreting organs and surfaces, as well as the secreted fluids. The solution used amounted commonly to between two drachms and half an ounce of the salt dissolved in four or eight parts of water; and it was introduced by means of a tube, stop-cock, and funnel, without pressure, by the simple sucking power of the vein. Blood was then drawn in successive portions every five, fifteen, or thirty seconds, sometimes from the opposite jugular vein, sometimes from the masseteric (maxillary) vein, sometimes from the external thoracic veins or the saphena vein, or from the metatarsal artery; and the ferro-cyanate of potass was sought for in the serum of the blood by means of a solution of sulphate of iron, followed by a drop or two of muriatic acid. By the same test it was sought for in the solids and fluids of the body immediately after death. The results are as follows:—

In from twenty to thirty seconds after the solution has been introduced into the jugular vein, the salt may be detected by a green, greenish blue, or azure tint on the addition of the test to the serum of blood from the jugular vein of the opposite side; in which case of course it must have traversed in that time the anterior vena-cava, the right heart, the pulmonary artery and veins, the left heart, the aorta, carotids and their ramifications.—In about the same period the salt will be found in the external thoracic veins; and on one occasion it was found in twenty seconds in the saphena vein.—It was occasionally found in the external maxillary artery of the opposite side in from ten to fifteen seconds; and in three experiments it was found in the blood of the metatarsal artery of the hindleg between the twentieth and fortieth seconds.—Some attempts were made to determine whether the celerity of motion thus indicated was increased by an increase in the action of the heart; but the experiments were not satisfactory.

The salt was secreted with great quickness, though not in large quantity, by the serous membranes; and those which are nearest the centre of the circulation secrete it soonest and in largest quantity. In different experiments, the time at which it began to appear on the serous surfaces varied from two minutes to four, seven, and even fifteen minutes. It was always most distinct in the serous sac of the pericardium, then in that of the pleura and peritonæum, and least distinct in the capsules of the joints. It could never be discovered in the ventricles of the brain.

Secretion takes place much more slowly from the surface of the mucous membranes. Still the salt was found, after a few minutes, on their free surface, and even sooner between the mucous membrane and the subjacent muscular coat. It was secreted more quickly from the right half of the stomach, than from the mucous coat of the intestines, less quickly still from the lining membrane of the air-passages, and still less quickly from the genital and urinary mucous membrane. It never could be discovered on any of those mucous membranes which are covered by an epithelium, such as the lining membrane of the mouth, gullet, or left side of the stomach. There the secretion of mucus is obviously very slow, if it takes place at all.

Among the glandular organs there are several, such as the liver, spleen, and thyroid gland, whose colour renders it impossible to apply the present method of investigation to them. In the salivary glands, however, it was clearly proved that the salt was eliminated with great celerity and in large quantity. In like manner, the kidneys were proved to eliminate it powerfully. Sometimes when the ferro-cyanate of potass had not been introduced into the blood of the jugular vein above a minute, it obviously existed in the cortical or tubular structure of the kidneys, or even also in the pelvis. It was not so soon conveyed into the urine, however; which is easily accounted for, as the urine descends but slowly from the pelvis of the kidneys along the ureters.

The salt was never so distinctly present in the lungs as might have been anticipated. Perhaps the colour of the lungs prevented the delicate application of the test. Perhaps the salt traverses the lungs so quickly that it adheres very slightly and in small quantity to the sides of the air-cells. It sometimes attaches itself rapidly to the inner membrane of the blood-vessels, more commonly not.

It passes with considerable rapidity into the lymphatic system. In one experiment it passed into the thoracic duct from the jugular vein in one minute. A remarkable fact is, that it does not appear so soon in the lymphatic ganglions as in the lymphatic vessels; on two occasions, none of the salt could be detected in the lymphatic glands, though it was obviously present in the lymph of the thoracic duct. *Dr Hering* concludes from that fact, that fluids pass not only from lymphatic vessels into veins, as some modern anatomists have demonstrated, but likewise from blood-vessels, probably, indeed, from arteries, into the lymphatic vessels.

Foreign substances introduced into the blood are rapidly eliminated, especially by the kidneys. This was well known to physiologists before; but the author points out the period within which this may be accomplished. In the instance of the ferro-cyanate of potass, he finds that the elimination commences in a few minutes; in the course of from five to eight hours it is no longer to be found in the blood; and in twenty-four hours it has disappeared also from the solids of the body.

MEDICAL PATHOLOGY.

On Inflammation of the Spleen ending in Suppuration. [*Répertoire Général d'Anatomie, &c.* 1829, *deuxième Trimestre, and Dr Tweedie's Clinical Illustrations of Fever.*]—Few cases of splenitis ending in suppuration are to be found in the records of medicine. The two following remarkable examples of it are therefore worthy of particular notice. The first occurred to *Dr Tweedie*, physician to the London Fever Hospital, in the course of his practice in that establishment; the other is related by *Dr Raikem* of Volterra in a late number of Breschet's Répertoire.

Dr Tweedie's patient was a girl, twenty years of age, who was supposed to have fever, and was on that account sent to the Fever Hospital when she had been seven days ill. Her chief complaint at the time of her admission was of thirst, want of sleep, and pain in the loins. There was no headach, pain of chest or belly, or cough; but the countenance was flushed, the tongue foul, the bowels loose, and the pulse very frequent. The parotid glands had begun to swell. Having been bled before admission, the treatment was at first confined to leeches applied over the parotids, and a mild course of mercury, united with ipecacuan. But eight days after she entered the hospital, she was attacked with pain on inspiration in the left side of the chest, corresponding with the lower part of the left lung, and hurried breathing, without cough. She had also tenderness of the belly. Bleeding, blistering, and the use of calomel and opium were followed by relief of the pain; but the respiration continued hurried, the pulse frequent, the general excitement considerable, and the belly distended and tender. She was in this stage of the disease attacked with erysipelas of the head and face, which ended fatally between seven and eight weeks from the commencement of her illness. "There was no disease in the brain or chest. On raising the left lung, a small tumour on the diaphragm appeared, which at first gave the impression of diaphragmatic hernia; on more close examination, however, it appeared to be an abscess, covered only by the pleura of the diaphragm. On tracing the origin of this abscess, it was discovered in the spleen, which adhered firmly to the diaphragm and cardiac orifice of the stomach. Two abscesses were formed in the spleen, one in its centre and another at the point of its adhesion to the diaphragm, the penitonæal covering and fibres of which it had destroyed, and thus protruded into the left pleural cavity. In the ileum there were several small ulcers, one of which had the appearance of undergoing the process of healing, being covered with a thin film of lymph, while its edges had assumed the level of the centre of the ulcers."

Dr Raikem's case occurred in the person of a stout young man, of a sallow complexion, much addicted to spirituous liquors. He had been affected for some weeks with pain in the left hypochondrium, when he was seized with stomach complaints, for which he took a saline mineral water as a laxative. Next day, when Dr Raikem first saw him, there was nausea, anorexia, and loaded tongue; deep-seated pain under the left false ribs increased on pressure; yellowish liquid stools, burning heat of skin, and a frequent vibrating pulse. At night a fit of rigor ensued, followed by heat, and next morning by a remission of the febrile symptoms. For some days afterwards the disease resembled a remittent gastric fever; so that, after some insignificant palliative treatment, his physician had recourse to bark. This checked the periodical exacerbations of the fever, which, however, continued in a milder degree, with want of appetite, nausea, occasional vomiting, diarrhœa, and increasing debility. At the same time there appeared in the left hypochondrium a hard elastic tumour, in which pressure excited lancinating pain. A considerable variety of heterogeneous remedies appear to have been resorted to almost at one and the same time, as soon as the nature of the disease thus developed itself; but no relief was obtained. The tumour first grew larger, then softened, and next became less; and when it began to diminish in size, it was impossible to examine it without exciting acute pain. The patient about this period also began to complain of lancinating and lacerating pain in the left lumbar region, where there was an œdematous tumour with deep-seated fluctuation. Afterwards the pain in this quarter became less acute while the fever increased, the abdomen became tympanitic, and the tumour extended from the ribs to the crural arch. The breathing was now short and laborious, the pulse hurried and feeble, the patient was tormented with a frequent dry cough, copious partial sweats broke out, and death at length ensued two months after Dr Raikem first saw him, the intellectual faculties having continued entire to the last. The left cavity of the chest contained two pounds of yellow serum containing some fibrinous flakes, and the pleura of the same side was covered with a thin layer of puriform fibrin, reddish and thickened. The corresponding lung was healthy in its internal structure, but its lobes adhered to one another, and to the parietes of the chest. The right lung and pleura were healthy. The liver too was healthy. The stomach and intestines were healthy outwardly, but the state of their inner membrane was not ascertained. The upper part of the descending colon, as well as the greater curvature of the stomach, adhered to the spleen. That organ was twice its ordinary size, and crimson red at its edges; and the portion of abdominal peritonæum corresponding with its posterior surface was raised by a white, thick, puriform and very fetid liquid. This proceeded from the interior of the spleen, which was hollowed out into one great abscess. The pus was consequently effused between the peritonæum on the hand, and the adjacent muscles, spine, and kidney; and it worked its way down to the crural arch. The fetid odour of the pus was so overpowering that the dissection could not be farther proceeded with.

Inflammation of the Spinal Chord producing Tetanus. [*Annali Universali di Medicina Febbraio e Marzo* 1828.]—A washerwoman, forty-four years of age, who was liable to slight rheumatic pains was attacked, after standing for some time in the water, with stiffness of the jaw, neck, and limbs, in consequence of which she was compelled to confine herself to bed. On the third day she entered the hospital of Udina. At that time the trunk was bent backward in a state of opisthotonos, and the arms were stiffly extended and compressed against the chest. The breathing was frequent and feeble; the pulse strong and frequent; the skin hot; the bowels constipated; the urine passed with difficulty; the intellectual faculties and sensibility entire; the thirst considerable; the tongue dry, and on the edge red. On the third day the locked jaw, which never equalled in violence the other spasmodic affections, had not increased; but the spasm of the trunk and arms was more violent, and the abdominal muscles were very rigid. The febrile state of the circulation was unaltered. The brain continued free, and the sensibility every where entire. On the fifth day she expired.

The brain and cerebellum were healthy. On the outside of the dura mater of the spinal cord there was some effusion of serosity, tinged with blood. The spinal arachnoid membrane was unaltered. But the pia mater presented anteriorly a degree of vascularity which did not exist on its posterior surface. The medulla oblongata was healthy. Immediately below the decussation of the corpora pyramidalia, the spinal cord presented in its whole anterior half a series of little spherical swellings, which retained their form and prominence after the pia mater was removed, and which varied in size from that of a millet-seed to that of a lentil. So long as the pia mater remained undivided, the cord appeared to retain its consistence; but as soon as the sheath of the pia mater was laid open, it was found that its whole anterior portion was converted into a soft pulpy substance formed of an infinite number of the small bodies already mentioned. The lateral longitudinal grooves of the chord were in consequence obliterated, and the middle groove much diminished. After a few hours' exposure to the air, the pulpy substance became soft like the starch of rice. The colour of the anterior part of the chord was dull white, inclining to yellow, but in the centre some red points could be seen. The posterior half of the chord was on the contrary healthy in colour, form, and consistence. By careful dissection it was farther found, that the medullary matter alone, and not the cortical substance, was affected. The anterior and posterior roots of the nerves presented quite a different appearance, the latter being quite healthy, while the former were very soft, of a yellowish-white tint, occupied by little elevations like the chord itself, and very easily torn away. There was no appearance in the organs of the chest or belly worthy of particular mention.

This interesting case shows that tetanic symptoms may he produced by true inflammation of the spinal chord, and illustrates excellently the discoveries of *Mr Bell* and *M. Magendie* on the separate function of the anterior and posterior roots of the spinal nerves. The tetanic spasm seems to have been continuous, and not liable to be increased in sudden violent paroxysms, as in proper tetanus. This character of the spasm we have remarked in a somewhat parallel case where the inflammation was confined to the cervical portion of the chord.

SURGICAL PATHOLOGY.

M. Dupuytren on the Aneurysmatic Dilatation of the Superficial Vessels of the Head, [*Répertoire Général d'Anat. et de Physiol. Pathologiques, T. vi. iime partie.* 1828.]—*Professor Pelletan of Paris*, *Mr Wardrop of London*, *Mr Maclure of Glasgow*, *and Mr Syme of Edinburgh** have each published an account of an operation for this extraordinary disease. From what is contained in their narratives, it appears that an aneurysmal state sometimes affects the whole course and ramifications of the temporal artery, or posterior artery of the ear,—that the remedy is the tying of the vessel which supplies the dilated arteries,—and that this disease is also generally accompanied with the developement of an apparently cellular structure, which is not so easily remedied as the dilated condition of the arterial branches, but commonly remains after the ligature of the supplying vessel, and becomes subsequently the source of fresh hemorrhage and danger. Of the four cases alluded to Mr Syme's alone was radically and completely cured; and this seems to have been mainly owing to his having cut out the cellular tumour altogether.

In the number of *Breschet's* Répertoire for October 1828 (published, however, less than a year ago,) another case of the same nature, which occurred in 1825, and therefore before any of those mentioned above, has been described by *Baron Dupuytren*. The aneurysmal dilatation affected the arteries of the ear, temple, and hind-head, and was accompanied by great thickening and enlargement of the ear. The patient was born with two small *nævi* on the outer fold of the *concha*, which appeared a little larger and thicker at these spots, but was not otherwise deformed. No change took place till he attained his twelfth year, and till then the only inconvenience he suffered was from a constant itchiness of the part, from which on scratching it florid blood flowed. But as puberty approached, the mark began to enlarge and assumed a violet colour. In three years the ear was twice as large as the other, and slight movements of pulsation were remarked in it. Eight months after that a troublesome hemorrhage, which could only be arrested by pressure, was occasioned by his hat having been roughly pulled off. At this time any unusual exercise, strong food, or wine, caused a very annoying increase of the pulsation; and a few months later a second serious hemorrhage occurred. Continuous pressure, which was now resorted to, procured only partial and temporary relief; when the pressure was discontinued, the tumour rapidly returned to its original size. Astringent applications were quite useless.

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* See the paper of the last mentioned gentleman in this Journal, xxxi. 68.

Two other attacks of hemorrhage having occurred, the patient entered the Hotel-Dieu of Sens, where the surgeons of the hospital tied successively the temporal, anterior auricular, and occipital arteries, without any permanent advantage. Several attacks of hemorrhage from the ear took place not many days after the last of the arteries was secured. An attempt was then made to tie the external carotid, the common origin of all the dilated vessels; but *M. Dupuytren* expresses his conviction that the surgeon had only tied the trunk of the temporal artery. After remaining three months in the hospital at Sens, he left it without having experienced any material amendment. Some time afterwards, when in his twentieth year, he repaired to Paris, and became the patient of *M. Dupuytren*.

At this time the diseased ear was twice as long as the other, as thick as the finger, and destitute of all appearance of the *helix* and *antehelix*. The posterior circumference presented a shallow groove, the consequence of an eschar formed during the previous treatment at Sens. "The whole ear had a deep violet colour, and was soft and compressible; the fingers could distinctly feel pulsation in some parts, and at other places movements of expansion and contraction isochronous with the pulse; and these movements communicated to the ear a general impulse, which alternately raised it from the head and approximated it. The whole occipital and temporal regions were bluish, and covered with numerous roundish elevations. Pressure on the primitive carotid caused for the time a rapid shrinking of the tumour and disappearance of the pulsation. The patient's health was excellent; his hearing and sight in the affected side perfect; and he was free from pain, but complained much of a troublesome rushing sound and heat in the diseased part.

It was evident from examination with the eye and the finger, that the disease consisted of two distinct elements. The one was composed of large, sinuous, knotty, uneven, full and compressible canals, ramified over the temple and hind-head like arteries, and equal in diameter at their trunk to the size of the little finger, at their smallest ramifications not less than a crow-quill. The course of these canals, and their violent pulsation, clearly showed that they were dilated arteries. The other element in the mass of disease plainly consisted in an *Erectile Tissue* of accidental formation, similar to that of the *corpus cavernosum penis*. This texture filled up the intervals between the dilated arteries, and communicated to the skin its blue colour. It was the filling of this with blood which produced the alternate expansion and contraction of the diseased part. The hemorrhage, which always followed even slight injuries of the part, evidently proceeded from the same erectile tissue.

M. Dupuytren, warned by the previous ill success of the surgeons at Sens, resolved to attack the whole disease at once by tying the common carotid. This was done by applying a single ligature. Immediately afterwards the pulsation and expansive movements of the tumour ceased, and the ear was much reduced in size. Some headach followed the operation, but next day abated. Slight vomiting and pricking pain of the ear also occurred, but were equally temporary. On the tenth day the erectile texture had shrunk to a third of its volume before the operation, and still no pulsation could be felt anywhere. Occasional blood-letting was practised on the appearance of slight excitement of the pulse. On the twelfth day the ligature came away from the artery. On the eighteenth day the erectile tissue presented slight signs of expansion synchronous with the diastole of the pulse; and on the thirtieth day the motion was obvious. The arterial dilatations, however, continued shrunk and free from pulsation. An attempt was then made to promote the further contraction of the erectile tissue of the ear by various kinds of pressure, and among the rest, by casing it with plaster of Paris. But no method was of any service. The ear remained steadily about a third of its size before the carotid artery was tied, and continued to expand with the contractions of the heart. But as the disease, in *M. Dupuytren's* opinion, ceased to be dangerous when unaccompanied with the dilated state of the arteries, and as the diseased mass was much too extensive to admit of being removed, the patient was dismissed.

LEGAL MEDICINE.

Obstetrical Statistics of the Kingdom of Würtemberg. [*Arch. Gén. de Méd.* xx. 76, from *Riecke's Beiträge zur geburtshülflichen Topographie von Würtemberg,* 1827.]—The most extensive and interesting document we have hitherto seen in the department of Obstetrical Statistics, is the account published not long ago of the chief particulars attending the births in the kingdom of Würtemberg for four years previous to July 1825, and drawn up by Dr Riecke from the lists returned by the various accoucheurs and midwives of that country. The report comprehends the extraordinary number of 219,355 deliveries, and 221,983 births.

In November 1823, about the middle of the period, the population of Würtemberg was 1,447,108, so that the average number of births was one annually in every 25.4 inhabitants. Of the births 51.5 per cent. were boys, and 48.5 per cent. girls.

The births are thus distributed through the different months of the year. In every thousand 90.5 are in January, 85 in May, 85.5 in September, 83.33 in February, 74 in June, 86.5 in October, 87.17 in March, 78.33 in July, 86.17 in November, 79 in April, 78.5 in August, and 86 in December. Of the births one in twenty were still-born, or died within twenty-four hours. In the circle of the Danube the proportion was one in 23, in that of the Jaxt one in 22, in that of Black Forest one in 21, and in that of the Neckar one in 19. Of the still births the proportion differed much in the different months; for of 1000, 93.5 were in January, 88 in December, 87 in October and in July, 85.5 in November, 85 in April, 84.5 in March, 78 in June, 77.5 in September, 77 in May, and 76 in February. In proportion to the births of each month, however, the numbers stand somewhat differently; so that July exhibits most still births, April comes next, and the last in order are September, May, and March. The proportion of legitimate to illegitimate births is eight to one. In the latter, the male births do not predominate so much over the female births, and still-births are more frequent.

The number of children are, on an average, seven for every mother, more numerous in the circle of the Danube, and less in the circles of the Danube and Black Forest. Of mothers who have above fourteen children, the average is one in 129; in the circle of the Danube it is one in 66, in that of the Black Forest one in 144, in that of the Jaxt one in 197, and in that of the Neckar one in 228. Still-births and miscarriages are most frequent in those who have an unusual number of children.

The average deaths in childbed are one in 173; but the proportion differed considerably in the different circles of the kingdom. In the circle of the Danube it was one in 135, in that of the Neckar one in 172, in that of the Jaxt one in 199, and in that of the Black Forest one in 210. The most frequent causes of death were hemorrhage, miliary and puerperal fever, hysteritis, metastasis of the milk, and general exhaustion; and a tenth of the deaths were owing to causes unconnected with the puerperal state. The deaths differ much in different months. Of 1000, 45.5 take place in August, 56 in June, 62 in July, 79 in October, 85 in March, 89 in April and November, 91 in September, 98.5 in February, 98.5 in January and May, and 108 in December.

The mortality among women in their first pregnancy is one in 143. Of these a fifth part are older than thirty, and among such the mortality is one in fifty. The mortality among women who have twins is one in fifty-seven or fifty-eight.

Miscarriages and premature deliveries amount to one in forty-one of the total deliveries. Of the premature births 54.5 are still-born, or die within twenty-four hours; and one-third of the total still-births are premature. Of the total births one-seventh are cases of primogeniture. In one delivery out of eighty-six there are more children than one, or in 1163 cases out of 100,000; and of these 1163, twins occur in 1146.9 cases, triplets in 15 2, and four children in 0.9. Of twins and triplets one-third died at birth. Of the children born four at a birth every one died soon after.

The labour was natural in 97.5 per cent of the cases. In the cases of natural labour the deaths in childbed were only one in 346. In the circle of the Black Forest they were only one in 500, in that of the Jaxt one in 419, in that of the Neckar one in 307, and that of the Danube one in 265. The still-births in natural labour were one in 31 over the whole kingdom, but in the circle of the Black Forest one in 26.5, in that of the Neckar one in 28, in that of the Jaxt one in 27.5, and in that of the Danube one in 32.5.—The number of labours in which assistance was required was 7,949 or 3.6 per cent. Such cases were most numerous in the circle of the Danube, and most rare in that of the Black Forest. In the cases in which assistance was required to finish the labour, one in 12.5 of the patients died, —but in the Black Forest one in 11.5, and in the circle of the Danube one in 14.5 only. In such cases one in 2.66 of the children died at birth,—only one in 3.66 in the circle of the Danube, and one in 2.5 in the circle of the Neckar. In cases where assistance was required, the child was a male in seven out of twelve; and in proportion to the number of each sex born in that manner, eight males died at birth for five females. It often happened that every male child of the same parent was still-born, while every female child survived. The greatest number of cases delivered artificially occurred in December, March, and February, the smallest number in August and October; February was to October as 13 to 9.4. In 1000 artificial deliveries, the forceps was used in 344 cases, turning and extraction by the feet was practised in 394, turning and extraction by the head in 2, extraction by the breech or feet in 63, the placenta was detached in 188, labour was brought on by force in 1, embryotomy was performed in 10.75, perforation of the cranium in 1.4, paracentesis of the cranium or abdomen for dropsy in 1.22, gastrotomy in 0.12, Cæsarean section in 0.25 during life, and as often after death, premature delivery in 0.25, and other artificial help was given in 32 cases.

From a comparison of the preceding facts with the state of obstetric practice in the four circles, it follows that artificial labour is most frequent, and the general mortality among mothers greatest where accoucheurs are most abundant; but that the mortality is least among cases where assistance is required to complete the labour, and that the number of still-births is also least, especially in artificial labours.

On the Mode of determining by the decay of a Body how long it has been in Water. [*Annales d'Hygiène Publique et de Médecine Légale, Octobre* 1829.] *M. Alphonse Devergie*, who has already distinguished himself by several interesting essays on various subjects connected with medical jurisprudence, has recently turned his attention to the appearances in the bodies of the drowned which indicate the interval during which they have remained in water. This is often an extremely important point to determine on coroner's inquests and precognitions, and no precise information has hitherto been possessed regarding it; while at the same time it appears from the researches of *M. Devergie* to be always susceptible of very exact determination from characters almost invariable in the order and rapidity of their developement. His researches were conducted by permission of the Prefecture of Police at Paris, at the Morgue of that city, the nature of which establishment we need scarcely stop to describe. His attention has been hitherto confined to the appearances developed at low temperatures, namely in the months of January, February, March, and April, when the mean temperature was 28¼, 35¼. 41, and 48 F. But he promises to give an early account of the modifications induced by increased temperature. His observations were made on 62 bodies, of which 45 were recognized by friends, so that the duration of their immersion in the water could be distinctly ascertained.

For the most part no change is observed before the 4th day. The rigidity which supervenes soon after death often continues till the second or third, sometimes till the fourth. On the *fourth day* the outside of the palms of the hands and the points and sides of the fingers begin to be blanched,—the backs of the hands and the rest of the body continuing unaltered.—About the *sixth or eighth day* the cuticle on the backs of the hands begins to whiten, as also that on the soles of the feet. At the same time the skin of the face is softened, and its whiteness is more dull and opaline than the rest of the skin.—*About the fifteenth day* the face is slightly puffed up and reddish here and there; a greenish tint appears over the middle of the sternum; the cuticle of the hands and feet is entirely white, with the exception of the upper surface of the feet; the cuticle of the palms has begun to shrivel; the subcutaneous cellular tissue of the chest is red; and the cortical substance of the upper part of the brain is greenish.—*In one month* the face is red and brownish, the eyelids and lips green and swelled, the cheeks greenish and also swelled, the parts around the eyes and nose brownish, the nose reddish-brown, swelled in women, but commonly flattened and depressed in men. The last point of difference depends on the circumstance that the bodies of females lie in water with their faces uppermost, and those of males with the face undermost. A reddish-brown patch, six or eight inches broad, and surrounded by a green border occupies the middle and upper part of the sternum. The scrotum and penis are enormously distended with gases. The cuticle of the hands and feet is extremely white and wrinkled, exactly like the effects of a poultice when long applied to a whitlow. The hair and nails still adhere firmly. The cellular tissue is very red. The lungs are very emphysematous, and extend over a considerable part of the pericardium. The windpipe no longer contains froth, which has been all expelled by the disengagement of gases in the lungs.—*In a month and a-half* besides the changes already mentioned, the neck and sides of the chest have acquired an intense green colour; the subcutaneous cellular tissue is still redder than before; and the cuticle begins to separate from the base of the hands; but the nails still adhere firmly.—*In two months* the whole body is covered with a slimy mud composed of extremely fine particles, and which oozes through the clothes. The period at which this appearance takes place is earlier in muddy water, and later in water which is much agitated. The face is much swelled, universally brownish, the lips very large and retracted from the teeth, the cuticle raised here and there in vesicles on the face, and every where easily detached, the course of the veins on the forehead strongly marked by blue lines. The greenness on the sides of the chest has extended upwards to the shoulders and downwards along the flanks to meet similar green marks proceeding from the groins; but the middle of the belly, as well as the arms, fore-arms, thighs, and legs retain their natural colour. The cuticle of the hands and feet is raised up and detached; and the nails are sometimes partially raised from the fingers, but adhere to the cuticle. The hair begins to drop away, and is easily plucked out. The cellular tissue is reddish-brown. The veins are distended with air and almost free of blood, but the arteries contain much bloody fluid. The windpipe is white over the cartilages, reddish-brown in the interstices between them. The heart is soft, flaccid, and empty; and if the right cavities contained blood at the time of death, the lining membrane of these cavities is jet-black, while that of the left cavities is reddish. The stomach and intestines are intensely red, like the effects of inflammation. A considerable quantity of bloody fluid is effused into the pleural and peritonæal sacs.—*In two months and a-half* the green tint of the skin is extended over the arms, fore-arms, and legs, and also in patches over the back. The nails of the fingers are commonly detached, but very rarely those of the toes. In females the state of the body is somewhat different at this period, in consequence of the greater abundance of fat under the skin. The skin of the face is white and opaline, and here and there eroded in small points. The breasts are much swelled, and of an opaline whiteness, while the nipples are brown, small, and flaccid. The belly is distended, also of opaline whiteness, together with the thighs, arms, and legs. The subcutaneous cellular tissue is converted into adipocire in the middle of the cheeks, under the eyebrows, on the chin, in the upper part of the neck, and very superficially in the breasts, fore-part of the thighs, and arm-pits. The muscles retain their natural colour and texture.—*In three months and a-half* the face is so altered, that the age of the person is not ascertainable, the scalp is destitute of hair and cuticle, is soft, and at the forehead thin or partially destroyed. The eyelids are in a great measure destroyed, the eyes sometimes protruded, sometimes shrivelled up, the skin over the cheeks and chin opaline, and covering a layer of adipocire, the lips depressed and saponified, the middle of the belly opaline, and interspersed with eroded spots. Erosions varying in size from that of a half-penny to that of a penny are dispersed over the skin of the arms and fore-arms, of a roundish form, and thin on their edge; and larger erosions of the same nature appear on the front of the thighs and legs. Those over the tibia lay bare the bone, which is red; and there the corrosions have a close resemblance to denuding ulcers formed during life. The cellular tissue at the bottom of these spots is saponified or converted into adipocire, which change, indeed, has also by this time occurred in the whole subcutaneous cellular tissue of the neck, groins, and fore part of the thighs. All the superficial muscles have acquired a rosy tint, and soft, flaccid consistence. The lungs no longer fill the chest, a serous fluid being effused into the pleural sac. The liver is softened, and of a greenish-brown tint, the gall-bladder empty, the intestines deep reddish-brown. *In four months and a-half* the scalp is detached from the bones, and almost entirely destroyed over the forehead, the eyebrows are also destroyed, and likewise the soft parts of the nose, and the lips are completely disorganized, so as to expose the whole alveolar part of the jaws. The skin of the neck and trunk is grayish-green, with black spots interspersed; that of the thighs is yellowish and firmer, and the skin in various parts presents a series of little, indurated, mammilliform elevations. The cellular tissue of the face, neck and fore-part of the thighs is completely saponified; elsewhere, it is distended by gases. The corroded spots of the skin are more extensive, and usually lay bare the whole tibia. The brain is partly putrified, partly converted into a matter resembling the adipocire of dead bodies.

M. Devergie has not yet had sufficient opportunities of ascertaining the changes occurring at precise periods later than four months and a-half. But the following general view of the ulterior transformations which the body undergoes are worthy of notice. In the body of a woman who appeared to have been from five months to five and a-half in the water, he found the whole head completely saponified, the cheeks so hard that they yielded with difficulty to the finger, and covered with a calcareous-looking deposit. The skin and muscles at the union of the neck with the trunk were destroyed; but a great part of the skin over the middle of the trunk was entire, having been protected by stays. This he has often had occasion to observe. The skin in many places was covered with little tubercles encrusted with calcareous salts, and in some parts hardened into a substance like parchment. In the body of another woman which had been apparently from ten to twelve months in water, the whole bones of the skull were bare and very brittle, the orbits filled with adipocire, the teeth gone, the fore-arms and hands gone, and the legs and feet reduced to the bones only. The skin on the fore-part of the body was so hard as to ring when struck with a key or scalpel, and covered with little hard tubercles, consisting of calcareous matter. The skin on the back was still soft, smooth, and free from tubercles. The whole cellular tissue was saponified, and was hard anteriorly, softer posteriorly. The muscles, which are contained in an aponeurotic expansion, retained their fibrous structure, the rest were saponified. The whole serous membranes had resisted decay, and had even become more solid than natural. The lungs were reduced to a tenth part of their volume, but were in a state of complete preservation, so that they admitted of being blown up to seven times the size of the space they occupied in the chest.

Among the appearances noticed in the preceding summary, two are especially worthy of notice,—the peculiar whiteness and wrinkling of the cuticle of the hands and feet,—and the slowness with which the green colour invades the rest of the body before it reaches the middle of the belly. These two characters will serve to distinguish a body which has putrified in the air from one which has decayed in water. In the air, the hands and feet never become wrinkled and white; but in water this change is brought on to such an extent, that if the body afterwards is allowed to putrify in the air, the earlier history of its decay may be long recognized by the state of the cuticle on those parts. As to the colour of the abdomen, this is the first part of the body which becomes green, when putrefaction takes place in the air; and the last where the greenness appears, if the body has remained in water.

Discovery of Arsenic in a body seven years after interment. (Archives Gén. de Médecine Décembre 1829.*)*—At the meeting of the Parisian Academy of Medicine on the 1st December last, *Professor Orfila* read an account of an important case in medical jurisprudence, which had been recently before communicated to him by *M. Ozanam*, chief physician of one of the hospitals at Lyons. The body had been buried in an elevated spot, and in a soil which absorbed moisture with great rapidity. Consequently it was found on disinterring it, that the coffin was entire, though dry and brittle, and that its sides were not even stained within, and the bottom only slightly stained with a brownish unctuous matter. The body was also entire; the head, trunk, and shoulders had preserved their form and position; but the internal organs of the chest and abdomen were destroyed, and there only remained a mass of soft brownish matter, which was deposited along the sides of the spine. In this matter *MM. Ozanam* and *Idt*, who were appointed judicially to investigate the case, discovered arsenic.

A part of this matter was boiled in repeated portions of distilled water till the water ceased to carry away with it any impregnation. The solutions thus obtained were subjected to two series of tests. 1. One portion was evaporated to dryness and the extract re-dissolved. The new solution on being reduced by evaporation to the volume of two pounds still retained a deep-red colour; on which account it was again evaporated to dryness, and deflagrated with nitre according to process of *Rapp* (See this Journal, vol. xx.) The saline residue being dissolved, filtered, boiled, with nitric acid, and saturated with pure potass,—the tests for arsenic acted on it in the usual manner. In particular, the hydrosulphate of ammonia, with a drop of muriatic acid, caused a yellow precipitate soluble in ammonia; and another portion of the solution being precipitated with nitrate of silver, the precipitate was mixed with pure vegetable charcoal and a little potass, and heated in a tube, when the tube became covered as it were with a slight tinning, which, when examined with the microscope of Delabarre, appeared to consist of metallic tetraedral crystals. When these crystals were placed in distilled water, and a stream of oxygen transmitted, they were dissolved, and the solution was acted on by the proper tests exactly as a solution of common white arsenic. 2. Another portion of the decoction of the original matter was treated with sulphuretted-hydrogen and a little muriatic acid; and the precipitate was reduced in a tube with charcoal and potass. A brilliant encrustation was procured, which when treated as in the former instance, became a solution of protoxide of arsenic, as the proper tests indicated.

This is an incident of extreme importance. It shows that in some circumstances it is hardly possible to assign a limit to the period after death within which arsenic may be detected by chemical analysis. Instances have occurred in this country where arsenic was detected several weeks after interment. In Germany it has been discovered thirteen and fourteen months afterwards (See Dr Christison's Treatise on Poisons, p. 261.) But no previous instance is on record of its detection after so lengthened an interval as in the case now described.

The method adopted by *MM. Ozanam* and *Idt* for determining the arsenical nature of the brilliant sublimates procured in their experiments is new. Doubts were expressed by several members of the academy whether oxygen transmitted through water, in which metallic arsenic was suspended, would have the effect of oxidating and dissolving it, as these two gentlemen alleged. But Professor Orfila, on repeating the experiment, found it perfectly correct. This might have been inferred from the fact that metallic arsenic is rapidly oxidated by contact with water in consequence of appropriating the oxygen held in solution by the water. The method here pointed out for converting the metal into the oxide is vastly inferior as a medico-legal test to the process proposed by *Dr Turner*, which consists in oxidating the crust and converting it into crystals of arsenious acid by the application of heat.

Erratum.—The name of the Gentleman who communicated the case of Arsenical Poisoning at page 61, we are requested to say, is not Ward but Wood.

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END OF VOLUME THIRTY-THIRD.

DEC 1 1918

No. CIV. will be published on the 1st of July 1830.

EDINBURGH:
PRINTED BY JOHN STARK,
Old Assembly Close.

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